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THE INFLUENCE OF DEGREE OF INTERPOLATED LEARNING ON RETROACTIVE INHIBITION AND THE OVERT TRANSFER OF SPECIFIC RESPONSES

By ARTHUR W. MELTON, University of Missouri, and
JEAN MCQUEEN IRWIN, Scripps College

In the analysis of retroactive inhibition and its relation to forgetting, one of the relationships most in need of precise determination is that between the amount and locus of the retroactive inhibition and the degree of learning of the interpolated activity. A knowledge of this relationship is essential to the interpretation of many other studies of retroactive inhibition, because other modes of variation in the interpolated activity frequently increase or decrease the degree of learning of that activity even though the frequency of repetition or the criterion of mastery has been held constant. In addition, the form of the relationship has particular significance for theories of retroactive inhibition, as will be shown later.

There have been four systematic investigations of this relationship. McGeoch found,[1] in a study of the learning of serial lists of 8 nonsense syllables by the anticipation method, that retroactive inhibition[2] (RI) as measured by recall scores

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[1] J. A. McGeoch, The influence of the degree of interpolated learning upon retroactive inhibition, this JOURNAL, 44, 1932, 695-708.

[2] The terms *retroactive inhibition* and *proactive inhibition* will be represented by the abbreviations RI and PI, respectively. Some abbreviation is necessary, and these seem preferable to "retroaction" and "proaction" which, especially in the case of "retroaction," emphasize the portions of the terms most subject to misinterpretation. By *retroactive inhibition* is meant merely the fact of poorer retention when some activity, usually a learning activity, has been interpolated between an original learning and the measurement of its retention, than when a period of comparative rest

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increased as the frequency of repetition of the interpolated list increased from 6 to 11, and remained relatively constant thereafter. On the other hand, when in the same experiment the RI was measured by the trials required to complete the relearning of the original list to 3 successive perfect trials, and the *Ss* were well practiced (Cycle II), it increased as the frequency of interpolated learning was increased from 6 to 11, remained constant with further increases in the frequency to 16 and 21, then dropped sharply as the frequency was increased to 26. In a second experiment,[3] with lists of 10 nonsense syllables which were learned for various amounts of time by the method of complete presentation, McGeoch found that study of the interpolated list for amounts of time beyond 150 sec. was accompanied by a decrease in the RI as measured by written recall. Lahey has recently used the method of complete presentation and written recall with lists of words to study this relationship,[4] and has verified McGeoch's conclusion only in the case of children with IQ above 105. Average and dull children show no decrease in the amount of RI when the interpolated material is studied for the longest time used in the experiment. She rightly notes, however, that this finding is not inconsistent with the hypothesis that the maximal amount of RI occurs when the interpolated activity is learned to an intermediate degree. A recent study by McGeoch,[5] in which the anticipation method was again used, is not relevant to the question of the effect of very high degrees of interpolated learning since such high degrees were not included among the work conditions.

Although in general these experiments suggest the truth of this hypothesis, there is no case in which the *decrease* in RI with increased learning of the interpolated material is statistically significant. Furthermore, different results were obtained when RI was measured by anticipation recalls in which only 2 sec. were allowed for the recall of each syllable, written recall in which several minutes were allowed for recall of the whole list, and by the trials required to relearn the list to a criterion of perfection. An hypothesis that permits a rationalization of these differences is, however, suggested by McGeoch's study with the anticipation method. If it can be reliably demonstrated that a high degree of learning of the interpolated activity reduces the RI as measured by trials to relearn to a criterion, but leaves unaffected the RI measured by the recall on the first relearning trial, this supports the hypothesis that the RI becomes localized in the early portion of the relearning process when the interpolated activity is overlearned. In other words, the persistence of the interference effects in the face of renewed reënforcements of the original activity becomes less as the degree of interpolated learning is increased from some intermediate degree

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has intervened. By *proactive inhibition* is meant either (a) the retardation of the learning of an activity when some other activity has occurred as a prior condition (sometimes called negative transfer), or (b) the fact of poorer retention of an activity when some other activity has occurred as a prior condition of the original learning of that activity, than when a period of comparative rest preceded the original learning.

[3] *Op. cit.*, 701-704.

[4] M. F. L. Lahey, Retroactive inhibition as a function of age, intelligence, and the duration of the interpolated activity, *Cath. Univ. Amer. Educ. Res. Monog.*, 10, 1937, 1-93.

[5] J. A. McGeoch, Studies in retroactive inhibitition: VII. Retroactive inhibition as a function of the length and frequency of presentation of the interpolated lists, *J. Exper. Psychol.*, 19, 1936, 674-693.

to some high degree, although the initial amount of interference, as measured by recall, may remain constant.

Verification of these hypotheses regarding the amount and the locus of the RI in serial verbal learning is given by the experiment to be reported. The design of this experiment follows closely the one with serial learning by the anticipation method reported by McGeoch.[6] Four differences were introduced in order to permit a detailed analysis of the recall and relearning processes: (1) a greater number of *Ss* were used; (2) a control condition was introduced to enable an evaluation of the

TABLE I

SEQUENCE OF EVENTS IN EXPERIMENTAL CONDITIONS DESIGNED TO MEASURE RI AFTER VARIOUS AMOUNTS OF INTERPOLATED LEARNING

Condition	Original learning	Rest	Interpolated learning	Rest	Relearning original list
I	5 trials		30 min. rest		to 2 perfect
II	5 trials	1 min.	5 trials (3.5 min.)	25.5 min.	to 2 perfect
III	5 trials	1 min.	10 trials (7 min.)	22.0 min.	to 2 perfect
IV	5 trials	1 min.	20 trials (14 min.)	15.0 min.	to 2 perfect
V	5 trials	1 min.	40 trials (28 min.)	1.0 min.	to 2 perfect
VI			Continuous learning to 2 successive errorless trials		

proactive inhibition (PI) of the interpolated learning by the original learning; (3) very difficult lists of nonsense syllables were used in order to prolong the relearning process for all *Ss*; (4) *E* recorded not only the correct anticipations and failures to anticipate during learning and relearning, but also the specific erroneous anticipations. In particular, the analyses of these erroneous anticipations have provided important corroborations and interpretations of the results obtained with the usual measures of retention and RI.

METHOD

The *Ss* were 24 college men who learned lists of 18 nonsense syllables under each of the 6 conditions outlined in Table I. In the 5 conditions that had directly to do with the relationship between the amount of RI and degree of learning of the interpolated material, the practice on the original list was held constant at 5 repetitions and the relearning was begun 30 min. after the completion of the fifth trial of learning. In the rest condition (Condition I) this 30-min. period was filled with the reading of humorous magazines, and all the shorter rest periods indicated in Table I were filled in this way. The 4 RI conditions were alike in that the first trial on the interpolated lists occurred 1 min. after the completion of the fifth trial of original learning. They differed in the number of trials spent in learning the

[6] *Op. cit.*, 696.

interpolated list and in the amount of rest between the last trial on the interpolated list and the first relearning trial on the original list. The sixth condition shown in Table I involved merely the continuous learning of a list to complete mastery or for 40 trials, whichever occurred last. One purpose of this condition was to provide a measure of the rate of learning of the interpolated lists when there was no prior learning of an original list for 5 trials, *i.e.* to provide a control for the PI of the interpolated list. A second purpose of this condition was to prevent the *S*s from becoming set for a cessation of the learning of the first list after 5 trials.

The syllables were presented at a 2-sec. rate on a pin-drive memory-drum powered by a G.E. synchronous motor, such as described by Ward,[7] and the anticipation method was used throughout. There was a 6-sec. interval of rest between trials, exclusive of the 2-sec. period during which the cue for the anticipation of the first syllable in the list was being presented. Special instructions were used to minimize common misinterpretations of the instruction "to anticipate" and thus to improve the reliability of intra-trial learning records. The obvious, though apparently infrequently recognized, need for such special instructions, and their importance for our detailed analysis of errors during learning and relearning, warrant the following verbatim statement of them.

(1) Alway spell out the nonsense syllable which you think will turn into the aperture next; do not pronounce it.
(2) Never attempt to anticipate a syllable before the one just preceding it has appeared in the aperture; that is, do not get ahead of the drum.
(3) Always check the accuracy of your anticipation by noticing the syllable that turns into the aperture.
(4) Always try to anticipate each syllable in the list, and try to anticipate as many as possible during each trial. You must not concentrate on a few of the syllables during the whole of one trial, then concentrate on a few more during the next trial, etc.; instead, you must study each syllable for the time that it is exposed. If you should be unable to anticipate all the syllables after only a few trials or if you fail to anticipate a syllable that you have correctly anticipated in previous trials, do not permit this to disrupt your learning process because such things are the rule rather than the exception. Under no circumstances should you give up trying to anticipate as many syllables as possible.
(5) If you anticipate a particular syllable, thinking that it will appear next in the aperture, but it does not appear and you think that it will appear on the next turn of the drum, you must call out the syllable again. In short, the syllable must be called out immediately before it turns into the aperture if it is to be counted as correct.
(6) If you are unable to remember a complete syllable, but can remember 1 or more letters of it, call out these letters since these partially correct anticipations are counted in your favor.

During the 3 preliminary practice days, *E* interrupted the learning at any time to point out a failure to follow these instructions.

Control of motivation is a major problem in every learning experiment in which the *S*s sometimes learn to a criterion of perfection and at other times for a fixed number of trials regardless of the degrees of mastery achieved. An attempt was made to establish a general task-set by the following instructions:

(1) You will continue your attempts to anticipate the syllables in the list until

[7] L. B. Ward, Reminiscence and rote learning, *Psychol. Monog.*, 49, 1937, (no. 220), 1-64.

you have satisfied the degree of perfection required of you or until you have studied the list for a number of trials which the experimenter will determine without your knowledge.

(2) Remember that the time you spend in this experiment is to some extent determined by the speed with which you learn the lists, and that the time you save is your profit if you get through before the hour is up. You are not being paid by the hour; you are being paid by the number of experimental sessions you complete. (This instruction was necessary because all *Ss* were paid for serving in the experiment.)[8]

(3) (Before the relearning of the original list) I am now going to present again the first list you learned today and you are to study it further and relearn it until I stop you. If you can recall any of its syllables during the first time it is presented, be sure to spell them out at the proper time. It is very important that you make a great effort to recall every syllable which you possibly can on the first trials. What you can remember on these first few times is very valuable for our records. Remember what your job is, and at no time fail to put forward your best efforts.

TABLE II

SAMPLE EXPERIMENTAL SCHEDULES TO ILLUSTRATE COUNTERBALANCING OF PRACTICE EFFECTS AND MATERIALS LEARNED

(Arabic numerals refer to the lists of nonsense syllables given in Table III; roman numerals refer to the experimental conditions described in Table I.)

S	Experimental day											
	Cycle I						Cycle II					
	1	2	3	4	5	6	7	8	9	10	11	12
1&13	2 VI	3–4 IV	5–6 II	7 I	9–10 V	11–12 III	14 VI	15–16 IV	17–18 II	19 I	21–22 V	23–24 III
2&14	1–2 IV	3–4 II	5 I	7–8 V	9–10 III	12 VI	13–14 IV	15–16 II	17 I	19–20 V	21–22 III	24 VI
Etc. for 3&15, 4&16, 5&17, and 6&18.												
7&19	14 VI	15–16 IV	17–18 II	19 I	21–22 V	23–24 III	2 VI	3–4 IV	5–6 II	7 I	9–10 V	11–12 III
8&20	13–14 IV	15–16 II	17 I	19–20 V	21–22 III	24 VI	1–2 IV	3–4 II	5 I	7–8 V	9–10 III	12 VI
Etc. for 9&21, 10&22, 11&23, and 12&24.												

The 24 *Ss* went through 2 complete cycles of the 6 experimental conditions after 3 days of practice during which they worked under conditions VI, I, and IV. The method used to control practice effects and differences in the difficulty of the lists is illustrated in Table II. The lists used during Cycle I with one group of 12 *Ss* were used during Cycle II with the second group of 12 *Ss*, thus permitting intercycle comparisons. It will also be noted that the odd-numbered lists were always used as lists for original learning and the even-numbered lists were always used for interpolated learning.

[8] We are greatly indebted to the Department of Psychology at Yale University and to the National Youth Administration at the University of Missouri for the funds used to pay these *Ss*.

Luh's rules,[9] for the construction of lists of syllables, could not be used in constructing 18-syllable lists. The following rules were substituted.

(1) All syllables must have association-values of less than 53.33%, as determined by Glaze.[10] The average association-values of the completed lists ranged between 24.5% and 29.3%.
(2) The letter *Y* was used neither as a vowel nor as a consonant.
(3) No vowel or consonant was repeated in any 4 consecutive syllables, and any letter used in the first syllable of a list was not used in the last syllable of the same list.
(4) Not more than 3 consonants occur twice as initial letters in syllables within the same list, and not more than 3 consonants occur twice as final letters. No consonant occurs more than 2 times as an initial letter, nor more than 2 times as a final letter, although a consonant may appear 4 times within the list.
(5) No 2 syllables within a list have 2 letters in common.
(6) The odd-numbered and even-numbered lists of a pair which is to be used on one experimental day, contain a minimal number of repetitions of 2 letters in the syllables in the same serial position.

The lists given in Table III were the ones used in the experiment, and the numbers are the ones referred to in Table II. On the memory drum the first unit of each list was preceded by the printed symbol, O-O-O, which served as the cue for the attempt to anticipate the first unit.

The inadequacy of the counterbalancing procedure for the control of practice effects in studies of RI and PI will be stressed in several connections during the presentation of the data. In the tables of data to be presented later there is ample evidence that the *Ss* were becoming more efficient in the learning of nonsense syllables during the first cycle of the experiment even though the 3 days of preliminary practice probably decreased this intra-cycle practice effect. When there is a residual practice effect from day to day in an experiment on RI or PI, it cannot be assumed that the level of practice of the *Ss* under the rest and work conditions has been equalized by the systematic rotation of the experimental conditions within the cycle. The reason for this is that relearning after the learning of an interpolated list must of necessity benefit from the fact that the *Ss* have had practice on 1 more list than under a rest condition in which no interpolated learning occurred. Similarly, in comparisons of learning with and without prior learning on the same day, such as those involved in determinations of PI, the measurements which the *E* expects to reveal negative transfer are always obtained when the *Ss* have reached a higher level of habituation to the task. In view of this, it may be maintained that experiments on RI and PI should be made only after the complete habituation of the *Ss* to the learning task if the specific inhibition is to be obtained without dilution by general positive transfer effects.

This dictum cannot, however, be applied without reservations because there is some evidence that susceptibility to RI decreases with repeated exposure of the *Ss* to such influences.[11] The investigator is, therefore, caught in a dilemma. If he studies RI with relatively naïve *Ss* he cannot equalize the general practice of the

[9] C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.
[10] J. A. Glaze, The association value of nonsense syllables, *Ped Sem.*, 35, 1928, 255-269.
[11] Cf. *e.g.* McGeoch, *op. cit.*, 701, and The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

Ss before the relearning under the rest and work conditions; if he uses *Ss* who have been thoroughly habituated, the *Ss* may have learned to avoid the inhibitory effect. The only solution the systematist can reach from these considerations is that rigid standardization of these basic conditions among various experiments must be made if integration of the results of different experiments is to be effected.

In our experiment, the data from Cycle II show, with few exceptions, greater absolute and relative amounts of RI than the data from Cycle I. Since the control of general practice effects in Cycle I may be considered inadequate, for the reasons cited above, the data from Cycle II will be given the greatest weight in our interpretations.

Learning of Original and Interpolated Lists

In this study the degree of learning of the original and interpolated lists has been controlled in terms of frequency of repetition under conditions of motivation known to produce learning, and since the total time per repetition was constant, the total time spent in learning was likewise subject to direct control. It is important, however, to determine whether this method of control produced a constant frequency of correct anticipations in the case of the original learning. In Cycle I the mean correct anticipations during the 5 trials or original learning were 16.4 $\pm$1.6, 16.8 $\pm$1.4, 16.4 $\pm$1.3, 16.3 $\pm$1.1, and 15.0 $\pm$1.6 for Conditions I through V, respectively, and in Cycle II the corresponding means were 21.3 $\pm$2.0, 20.6 $\pm$2.0, 21.9 $\pm$1.6, 20.0 $\pm$1.5, and 21.5 $\pm$1.3. The largest difference in Cycle I was 1.8 $\pm$1.2, and the largest difference in Cycle II was 1.9 $\pm$1.4.[12] The variations in the mean correct anticipations on the fifth learning trial were also insignificant. In Cycle I these means were 5.8 $\pm$0.6, 6.3 $\pm$0.5, 5.7 $\pm$0.6, 6.0 $\pm$0.4, and 5.8 $\pm$0.6 for Conditions I through V, respectively, and in Cycle II the corresponding means were 7.6 $\pm$0.6, 7.9 $\pm$0.8, 7.7 $\pm$0.6, 7.3 $\pm$0.5, and 8.0 $\pm$0.5.

As would be expected, the correct anticipations were not evenly distributed over the whole original lists. Thus, when the data from Conditions I through V were combined, the mean number of syllables correctly anticipated 0, 1, 2, 3, or 4 times was 11.1, 2.0, 1.8, 1.6, and 1.6, and 1.5 during Cycle I, and 9.3, 2.4, 2.2, 2.0, 2.1 during Cycle II. In short, more than 50% of the syllables in the original lists were not correctly anticipated at any time during original learning.

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[12] All measures of reliability are sigma. All sigma of the mean differences have been corrected for the correlation between the arrays. For this experimental design there are 23 degrees of freedom, and a t value of 2.807 is taken as an indication of a very significant mean difference. Cf. R. A. Fisher, *Statistical Methods for Research Workers*, 1934 (5th ed.), 158.

In Table IV we have presented 3 indices which serve to reveal the level of mastery of the interpolated lists after 5, 10, 20, and 40 trials. It should be noted that 5 and 10 repetitions of these lists produced only partial learning for all but 2 *Ss*; that almost all of the syllables were correctly anticipated at least once by every *S* when the lists were learned for 20 trials, and that 40 trials produced a high degree of learning in all *Ss*.

The most significant results in Table IV, are, however, those that

TABLE III

LISTS OF NONSENSE SYLLABLES OF LOW ASSOCIATION VALUE

(The average association value of the syllables in each list, as computed from Glaze's values, is given in parentheses.)

1 (25.2)	2 (26.3)	3 (26.7)	4 (26.3)	5 (26.7)	6 (24.5)	7 (26.7)	8 (25.9)
PUK	GOX	VAW	MOF	NAF	MEC	XAR	ZIN
ZAC	JEP	DUF	KEB	VUK	GIX	KEH	CUW
NOL	VID	GEJ	GIZ	PEQ	TUD	BIW	QOD
XIF	FUB	JOH	NAL	BIH	NOJ	GOJ	KAB
BUJ	ZAH	RIX	HUQ	DAJ	WEF	VUC	SIF
QAP	TEG	ZUB	FEP	RUZ	QIB	MAF	MEP
WEZ	PIW	MAQ	GOC	NIW	KUG	XIN	XUG
CIH	KUD	TOC	BIV	HEG	VOP	PEZ	JAL
VUT	FOJ	SIJ	XAZ	MOJ	ZEL	WOS	FOH
DOQ	XEC	RUW	LEQ	DUP	WIJ	DUH	WEQ
KAM	NIQ	FAZ	WOH	KIF	XAT	KIQ	VUN
LEH	TUV	QET	JIK	XEB	CUQ	GEB	DAC
GIW	BOP	GIH	ZUP	QAR	GED	NAW	XOB
ZOB	CAX	WOJ	CEX	ZOD	ZIF	XUM	QIH
DAF	ZEQ	XUV	QAB	JUH	XOH	JIV	ZUM
XUQ	KIG	KEC	MIW	WEM	QAS	ZOF	GAW
CEJ	WUH	QIF	TOZ	XIC	TEJ	CEQ	VOC
ZIL	PAF	ZON	VUX	QOV	BUV	WUK	JEX

9 (26.7)	10 (25.6)	11 (27.0)	12 (29.3)	13 (26.7)	14 (26.3)	15 (26.7)	16 (26.7)
JOD	NIH	XAL	KEZ	BUP	FAP	KOJ	ZEC
QIM	FUP	WIK	QUB	FAJ	TIJ	BEW	JUW
PEX	VAD	HUZ	VOH	KEV	QOC	LAH	VIB
RUC	ROJ	VEC	JIW	DIH	DUZ	VIG	KOG
KAJ	GEW	GOQ	NAX	QOS	HEB	ZUR	ZAL
ZIT	QUC	TAJ	KUF	XUW	XIV	MEF	PEM
SEH	KAL	BIP	CEG	JEG	GUF	QOL	FIW
XUB	TIV	XES	ZIQ	NIV	ZOP	TIH	COJ
VOF	HEP	CUG	WOB	KOR	CEK	XAP	NUX
LIW	NUB	JOM	HUJ	ZAB	NAQ	JEC	HAQ
DEQ	CAZ	QID	LEX	MUQ	SIH	GUD	TIZ
ZAM	DIQ	TEF	PAQ	LIJ	PUW	NIZ	JEK
NUK	KEX	RUK	TOF	CEF	XOJ	TAW	BOF
GOW	VUH	ZAP	RIH	VAP	FEQ	VOB	XAC
JIC	ZOW	GIJ	SEJ	ZUH	GID	SUQ	PID
QUS	QAJ	XOF	WUP	WIB	KAH	KED	GUQ
REZ	GIC	MUW	KOC	XOL	MUX	XIM	VEH
TAH	XUL	NEQ	GAF	KAG	ZER	ZAV	MOX

TABLE III *Continued*

17 (27.0)	18 (27.4)	19 (26.7)	20 (26.7)	21 (25.6)	22 (26.7)	23 (26.7)	24 (28.1)
MUN	QAM	GOK	VAQ	XIP	NEF	NID	CUH
VOJ	VEB	RUX	KEM	LUB	TOV	ZUC	BAJ
HES	JUC	JEZ	POH	WAH	LIH	PEJ	GEV
QAT	XOR	TIS	BIX	QEM	BAZ	XOT	KIX
RIW	ZAN	FAQ	LUD	TOJ	QUN	QAV	WOQ
MOG	TIW	XOM	ZEG	CIZ	GEK	MIF	PAH
BEP	CES	DEH	WAP	RUV	DIW	CEB	LEB
ZUF	FOV	KUW	NOF	KEF	XOP	WUS	VUM
VIQ	HAJ	FIP	DIJ	XOD	TUZ	HOZ	NIR
TOH	GUK	LOJ	SEB	ZAT	SAJ	LIX	QAF
DAK	ZED	GAX	QUZ	NUW	MIG	VEF	ZOL
GEX	MIP	HUC	VOM	MIV	LOQ	QUP	XET
PUV	XAW	DIB	XAG	FOQ	XUR	SOJ	VIH
CIJ	KOB	LER	NEH	GAJ	BEJ	KIW	QUG
ZAS	QEN	ZOT	ZIR	CEH	CIK	NAC	POB
QOB	JID	VUQ	QOW	SIW	GAQ	XUH	MEJ
WUG	VAF	JIH	LAJ	ZUK	ZOH	MEQ	DAX
KER	TUH	XEW	FUH	DAQ	WUB	WOG	ZIW

reveal the proactive inhibition of the interpolated learning by the prior original learning. Whereas, 5 trials of original learning produced 16.4 correct anticipations during Cycle I and 21.3 during Cycle II, 5 trials of interpolated learning produced only 12.3 and 16.5 correct anticipations. If we compare the mean correct anticipations in each of the first 10 trials

TABLE IV

INDICES OF THE DEGREE OF LEARNING OF THE INTERPOLATED LISTS

	Cycle	Interpolated frequencies			
		5	10	20	40
Mean total correct anticipations	I	12.3	57.1	192.0	508.8
	II	16.5	71.9	219.0	557.8
Mean syllables correctly anticipated one or more times	I	6.0	13.2	17.0	18.0
	II	7.5	14.7	17.7	18.0
Number of Ss achieving one perfect trial	I	0	0	11	21
	II	0	2	17	24

of continuous learning of a single list under Condition VI with the same measures for interpolated learning (Conditions II, IV, V), the PI effect of 5 trials of original learning is shown to be transitory. The essential data are presented in Table V. In Cycle I the PI decreased steadily until the sixth trial, and was followed by an apparent facilitation effect; in Cycle II the PI decreased steadily until the seventh trial, and was not followed by a facilitation effect. No significance other than methodological should be attached to the facilitation effect in Cycle I, because it probably reflects merely a net difference in the general practice effect

resulting from the unavoidable difference in the number of lists learned by the *Ss* before learning an original and an interpolated list. When the *Ss* are thoroughly habituated to the learning task, as in Cycle II, the facilitation effect does not appear. On the other hand, the significance

TABLE V

MEAN CORRECT ANTICIPATIONS ON TRIALS 2 THROUGH 10 WITHOUT PRIOR LEARNING OF ANOTHER LIST AND AFTER 5 TRIALS ON ANOTHER LIST

Cycle	Condition of learning	Successive trials during learning								
		2	3	4	5	6	7	8	9	10
I	Without prior	1.92	3.54	4.67	5.96	6.92	7.46	8.08	8.88	9.83
	With prior	1.11	2.38	4.08	5.72	6.78	7.97	8.90	9.70	10.63
	% PI	−42.2	−32.8	−12.6	−4.0	−2.0	+6.8	+10.1	+9.2	+8.1
II	Without prior	2.13	4.38	5.54	7.38	8.79	9.58	10.96	11.46	12.50
	With prior	1.42	3.18	4.68	6.64	8.50	9.54	10.83	11.82	12.48
	% PI	−33.3	−27.4	−15.5	−10.0	−3.3	−0.4	−1.2	+3.1	−0.2

of the differences in the early trials that we have attributed to the PI cannot be questioned. In Cycle I the ratios of the differences to their standard errors in the case of trials 2 and 3 are 2.79 and 3.87, respectively, and the corresponding ratios in Cycle II are 2.54 and 3.64. None of the other differences is significant. In a later section, evidence for overt proactive transfer will be presented.

INTERPOLATED LEARNING AND RETROACTIVE INHIBITION

As previously indicated, in a study of serial learning by the anticipation method, McGeoch was unable to show a reliable decrease in the amount of RI when the interpolated material was greatly overlearned.[18] There was a suggestive decrease in the number of trials, but this was not paralleled by the recall scores (the number of correct anticipations on the first relearning trial). Our results verify and reconcile these apparently contradictory results with recall and relearning scores, and at the same time substantiate unequivocally the hypothesis that overlearning of the interpolated material tends to reduce the interference with the recall and relearning of the original material.

In Table VI are shown the mean trials required by the *Ss* to complete the mastery of the original list to criteria of 1 errorless trial and 2 successive errorless trials. The criterial trial or trials have not been included. Our primary concern is the comparison of the trials to relearn under the work and rest conditions, and the percentages of inhibition under work conditions have been computed by the formula 100 (Rest−Work)/Rest. In both cycles of the experiment the maximal inter-

[18] *Op. cit.*, this JOURNAL, 44, 1932, 695-708.

ference occurred when the interpolated list was learned for 10 or 20 trials and the minimal interference occurred when the interpolated list was learned for 40 trials. The only differences between the means which are at least twice their sigma, after correction for the correlation between the arrays,[14] are found in the relearning to 1 errorless trial in Cycle II. In this case the ratios of the differences between the rest condition and the 4 work conditions and their respective sigma are 1.57, 2.62, 3.43, and 0.20. Of greatest importance, however, is the fact that the difference between the means of the relearning trials after 20 interpolated trials and after 40 interpolated trials is 2.60 times its sigma.

Although this evidence for a decrease in the RI with an increase in the degree of interpolated learning is not of unquestionable reliability,

TABLE VI

DEGREE OF INTERPOLATED LEARNING AND RETROACTIVE INHIBITION AS MEASURED BY TRIALS TO RELEARN TO 1 PERFECT AND 2 SUCCESSIVE PERFECT TRIALS

	Condition	Continuous	Rest	Trials on interpolated list			
				5	10	20	40
Trials to 1 Perfect	RL Trials, Cycle I	16.2±1.7	19.5±1.9	19.8±1.9	21.0±1.7	19.3±2.0	18.6±1.8
	RL Trials, Cycle II	13.0±1.5	12.5±1.3	13.6±1.5	14.6±1.2	14.0±1.3	13.3±1.2
	% RI, Cycle I			1.5	7.7	+1.0	+4.6
	% RI, Cycle II			8.8	16.8	19.2	+1.6
Trials to 2 Perfect	RL Trials, Cycle I	23.6±2.4	25.8±2.7	25.5±2.8	27.6±2.7	25.4±3.2	24.4±2.7
	RL Trials, Cycle II	16.3±2.0	18.0±2.1	17.4±1.8	18.1±1.7	18.5±1.9	16.5±2.0
	% RI, Cycle I			+1.2	7.0	+1.6	+5.4
	% RI, Cycle II			+3.3	0.6	2.8	+8.3

the close approach in the case of relearning scores is made more significant by the evidence for the transitoriness of the RI during relearning under all work conditions. It will be noted in Table VI that the retroactive inhibition is consistently less when measurement is in terms of the trials required to relearn to 2 successive errorless trials—the more stringent criterion.

The characterization of RI as a transitory phenomenon is given further substantiation by the recall scores. As shown by a comparison of Table VII with Table VI, the percentages of RI as measured by recall are many times greater than the percentages of RI as measured by relearning scores.[15] But there is an added complication. The evidence for a reduc-

[14] The product-moment *r*s between the trials to relearn to 1 errorless trial under the rest and work conditions averaged 0.668 in Cycle I and 0.742 in Cycle II. The corresponding average *r*s for trials to relearn to 2 successive errorless trials were 0.647 and 0.653. These are averages of the 10 possible intercorrelations of 5 conditions. There was no systematic variation in the *r*s as the degree of interpolated learning increased.

[15] It should be noted that McGeoch (*op. cit.*, 698-699), obtained percentages of RI in relearning which were nearly as large as his percentages of RI in recall. Although the present percentages of RI in relearning have been computed directly

tion in the amount of RI as the interpolated trials were increased from 20 to 40 is not nearly so clear-cut in the case of the recall scores as it was in the case of the relearning scores. There is a slight increase in the number of correct anticipations at 40 trials, which is more pronounced in Cycle II than in Cycle I, but these differences in Cycle I and II are only 0.59 and 1.88 times their σ and are insignificant in comparison with the increases in the RI as the number of interpolated trials was increased from 5 to 20. In short, our data conform with those of McGeoch in that the tendency for a decrease in RI with overlearning of the interpolated material is more pronounced in the case of relearning scores than in

TABLE VII

DEGREE OF INTERPOLATED LEARNING AND RETROACTIVE INHIBITION AS MEASURED BY RECALL

Condition	Continuous	Rest	Trials on interpolated list			
			5	10	20	40
Recall, Cycle I	6.92±.59	3.50±.56	1.50±.27	0.71±.15	0.50±.18	0.67±.24
Recall, Cycle II	8.79±.77	5.58±.69	2.04±.36	1.33±.38	0.92±.25	1.54±.30
% RI, Cycle I			57.1	79.7	85.7	80.9
% RI, Cycle II			63.4	76.2	83.5	72.4

the case of recall scores, even though our data are more consistent than McGeoch's in showing such a decrease when either measure is used.

These apparently divergent relationships for recall and relearning scores may, however, be made intelligible if it is assumed that the transitoriness of the RI varies with the degree of learning of the interpolated list, being more transitory—of less duration in the relearning process and, therefore, less likely to affect the relearning scores—the greater the degree of interpolated learning. This assumption has been verified, in the first instance, by determining the average number of correct anticipations (an inverse measure of RI when compared with performance after rest) on the first, second, third, fourth, etc., relearning trials under the various work conditions. As shown in Fig. 1, the tendency for the amount of RI to increase and then decrease as the degree of interpolated learning is increased becomes more pronounced in successive relearning trials after the first (recall) trial. Apparently, the RI disappears most rapidly when 40 interpolated trials are given, and next most rapidly with 20 interpolated trials. In Cycle II the ratios of the difference between the 10- and 40-trial work conditions in relearning trials 2, 3, and 4 to their respective sigma were

from relearning trials, rather than from saving scores,' this fact is not responsible for the departure from McGeoch's results. Several points in the theory of RI presented at the end of this paper suggest that this difference occurred because McGeoch used a higher degree of original learning in his study.

2.45, 2.91, and 2.34. In Cycle I the corresponding ratios were 1.94, 1.22, and 1.31.

Analysis of the transitoriness of the RI in terms of mean correct anticipations during any one relearning trial must be limited to a few trials because some *Ss* achieve the criterion of mastery after only a few trials. A modification of the Vincent-Kjerstad technique such as employed

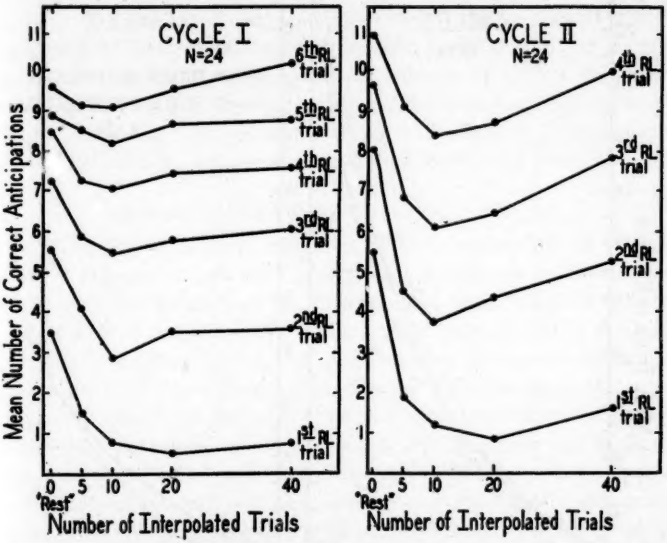

FIG. 1. RELATIONSHIP BETWEEN THE MEAN NUMBER OF CORRECT ANTICIPATIONS IN SUCCESSIVE RELEARNING TRIALS AND THE DEGREE OF LEARNING OF THE INTERPOLATED MATERIAL

The series of curves shows the more rapid disappearance of the RI with 40 interpolated trials than with 10 or 20 interpolated trials.

by Melton in studies of the form of the learning curve,[16] may be used to study the entire relearning period of each *S*. Thus, for the rest and work conditions we have computed the mean relearning trials required by the *Ss* before they could correctly anticipate 1, 2, 3, 4 . . . 18 units within a single trial. Only the results for the 10- and 40-trial work conditions and the rest condition have been shown in Fig. 2, since the curves for the 5- and 20-trial work conditions follow closely the curve for the 10-trial work

[16] A. W. Melton, The end-spurt in memorization curves as an artifact of the averaging of individual curves, *Psychol. Monog.*, 47, 1936, (no. 212), 119-133.

conditions. It is apparent that the RI persists throughout the relearning of the original list after 10 interpolated trials, in the sense that there is greater difficulty in relearning to every criterion of partial mastery. On the other hand, with 40 interpolated trials the *Ss* show greater difficulty in relearning only until they have reached a criterion of 12 or 13 correct

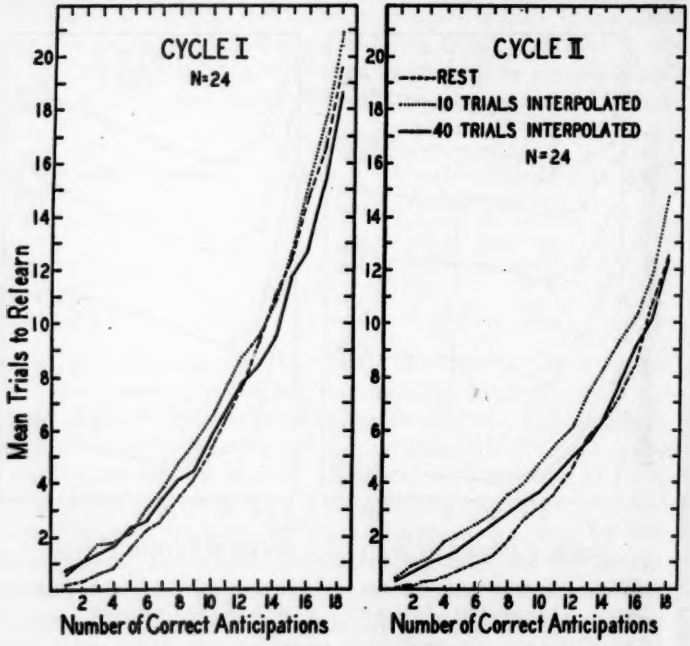

FIG. 2. MEAN TRIALS TO RELEARN THE ORIGINAL LIST TO VARIOUS LEVELS OF MASTERY
The RI after 40 interpolated trials is shown to be transitory.

anticipations out of the 18. From that point on to the attainment of the criterion of 1 errorless trial there is no difference between this work condition and the rest condition. That the ease of mastering the last part of the list is apparently greater under the 40-trial work condition than under the rest condition in Cycle I, must again be discounted because it probably reflects the general practice effect from the learning of the interpolated list.

Integration of the various lines of evidence which have been presented unquestionably supports the hypothesis that the maximal inhibition of an

original learned activity occurs when the interpolated activity is learned to a moderate degree. Very low or very high degrees of learning of the interpolated material give the smallest amounts of RI. The apparent discrepancy between the relationships obtained when the RI is measured by recall scores and when it is measured by relearning scores is to be attributed partly to a general tendency for the RI to disappear as the relearning of the original material proceeds toward complete mastery, and partly to the more rapid disappearance of the RI during relearning when the degree of interpolated learning is high. A decrease in the RI with overlearning of the interpolated material may not be evident in the first relearning trial but will be clearly shown in the second or third relearning trial because the RI disappears so much more rapidly with the overlearned material than it does with moderately learned interpolated material.

Specific Inter-List Interference Effects

Of theoretical importance in studies of RI is the analysis of evidence of active competition between the original and interpolated response systems during the learning of the interpolated material and, particularly, during the recall and relearning of the original material. For, if a syllable from the interpolated list occurs as a response during the attempt to recall and relearn the original list, there is *prima facie* evidence that active competition of the two response systems is occurring and this behavioral fact must be considered in explanations of RI. McKinney and McGeoch have recently reported such intrusions of interpolated word responses during the written recall of an original list of words,[17] and a corresponding but less pronounced tendency for words learned in an original list to intrude in the immediate written recall of interpolated words. No attempt, however, has heretofore been made to observe such intrusions during learning and relearning by the anticipation method, nor to discover the relationship between the frequency and conditions of occurrence of such intrusions and the degree of learning of the interpolated material.

In order to make such data available in this experiment, every incorrect response made by the *Ss* during the original learning, interpolated learning and relearning of the original material was recorded. All records were then searched to locate those instances in which an entire interpolated syllable was spelled out during the relearning of the original list and in which a syllable from the original list was spelled out during the learning of the interpolated list. Since some syllables which were identi-

[17] Fred McKinney and J. A. McGeoch, The character and extent of transfer in retroactive inhibition: Disparate serial lists, this JOURNAL, 47, 1935, 409-423.

fied as intrusions might have occurred as errors even though no interpolated (or original) list had been learned, 'control' tabulations were necessary. The control for the intrusions of interpolated syllables during the relearning of the original list was obtained by noting the number of 'intrusions' during relearning after rest (Condition I). In this case, an 'intrusion' syllable was one that appeared in the list which would have been interpolated if a work condition had been scheduled for that day. The control for the intrusions of original syllables during interpolated learning was obtained by noting the number of 'intrusions' during the continuous learning of a list without prior learning (Condition VI). In this case, an 'intrusion' syllable was one that appeared in the list which would have been given as original learning if such original learning had been scheduled. The arrangement of the schedule of lists and conditions which permits these control measurements has been illustrated in Table II.

The inter-list intrusions were classified according to the apparent source of the confusion. When a syllable in serial position n in an interpolated list displaced a syllable in position n or $n\pm1$ in an original list, and neither the displaced syllable nor the syllables in adjoining serial positions had 2 letters in common with the intruding syllable, the intrusion was said to be based on identity of serial positions. When a syllable in serial position n in an interpolated list displaced a syllable in a position farther removed than $n\pm1$ in the original list, and the displaced syllable or the syllable in the adjoining serial positions had 2 letters in common with the intruding syllable, the intrusion was said to be based on formal similarity of the materials learned. When a syllable in position n in an interpolated list displaced a syllable in position n or $n\pm1$ in an original list, and the displaced syllable or a syllable in an adjoining serial position had 2 letters in common with the intruding syllable, the intrusion was said to be based on both identity of serial position and formal similarity. A fourth class includes all those intrusions which failed to satisfy the criteria of the first three classes.[18]

A further problem arises in the tabulation of the frequency of intrusions, since the same intrusion syllable is sometimes repeated several times in succession during a single trial, and the same intrusion syllable may be repeated during several successive trials or intermittently through-

[18] This analysis of inter-list intrusions is obviously crude, because the pairs of lists used in the RI experiment must be constructed according to special rules if the factors of serial position, formal similarity, and meaningful similarity are to be isolated. Such pairs of lists are being used in studies now in progress.

out the relearning process. Although the very occurrence of such intra-trial and inter-trial repetitions is significant, our data are too few to permit their adequate treatment, and we have chosen the number of *different* intrusion syllables as the most stable and significant measure for our present purposes. In all the detailed analyses to follow, the *first* occurrence of an intrusion syllable during the relearning process determines the serial position of the displaced syllable and the trial during which the intrusion is said to occur.

The data on the frequency of overt intrusions of interpolated syllables during the relearning of the original lists are presented in Table VIII.[19]

TABLE VIII

FREQUENCY OF OVERT INTRUSIONS OF INTERPOLATED SYLLABLES DURING THE RELEARNING OF THE ORIGINAL SYLLABLES
(Both cycles combined)

Intrusions	Trials on interpolated lists				
	Rest	5	10	20	40
Total	9	65	92	53	58
Different syllables	6	45	64	37	43
Based on ser. pos.	0	22(48.9%)	39(60.9%)	18(48.7%)	11(25.6%)
Based on similarity	3	8(17.8%)	14(21.9%)	10(27.0%)	24(55.8%)
Based on ser. pos. and similarity	1	4	2	2	4
Unclassified	2	11	9	7	4

Of primary importance is the fact that relatively few 'intrusions' occur during the relearning after rest; whereas, many intrusions occur during relearning after any amount of interpolated learning. Furthermore, the frequency with which the intrusions occur during relearning after various amounts of interpolated learning roughly parallels our findings regarding RI as measured by recall and relearning scores. The maximal number of

[19] The data of both cycles of the experiment have been combined in making this analysis of intrusions. There is, however, an interesting shift in the frequency of intrusions with practice which will have considerable significance if it is verified by later experiments. During Cycle I the total number of different intrusion syllables during relearning after 5, 10, 20, and 40 interpolated trials was 18, 28, 20, and 25. During Cycle II the corresponding frequencies were 27, 36, 17, and 18. When repeated intrusion syllables were counted, the frequencies for Cycle I were 23, 38, 31, and 32, and the frequencies for Cycle II were 42, 54, 22, and 26. Thus, the number of intrusions during relearning increased with practice when there was a low degree of interpolated learning and decreased with practice when there was a high degree of interpolated learning. The frequencies of intrusions during the first relearning trial failed to show this relationship to practice; instead, the frequencies increased with practice under all RI conditions. Thus, in Cycle I there were 10, 15, 3, and 0 different intrusion syllables in the first relearning trial, and in Cycle II there were 18, 17, 8, and 3 such intrusions. When repeated intrusion syllables were counted, these frequencies were 12, 18, 4, and 0 in Cycle I, and 24, 20, 10, and 3 in Cycle II.

intrusions occurs when the interpolated list is learned for only 10 trials, and the number of intrusions after 5 interpolated trials and 40 interpolated trials is approximately the same. Since there was no significant change either in the number of different *Ss* showing intrusions under the 4 RI conditions, nor in the number of different relearning records (N = 48) showing intrusions, it may be concluded that the change in the frequency of intrusions as a function of the degree of interpolated learning was not the result of a selective sampling effect. The maximal total number of intrusions given by any *S* during the 8 relearning periods after interpolated learning was 17, the minimal number was 2, and the mean and S.D. were 7.9 and 4.2, respectively. The median number was 7.5 intrusions. Such intrusions are clearly a part of the normal process of relearning an original list after the learning of an interpolated list, under the conditions of this experiment.

Two sources of the confusion which leads to such intrusions are likewise clearly indicated by the data in Table VIII. Intrusions based on identity of serial position, as previously defined, increased in frequency absolutely and relatively as the trials on the interpolated list were increased from 5 to 10, and declined precipitously with further increases in the degree of learning of the interpolated list. On the other hand, the intrusions based on formal similarity of the syllables increased as the degree of interpolated learning was increased. These diverse relationships between the frequency of intrusions and the degree of interpolated learning suggest that the specific relationship between RI and degree of interpolated learning, as revealed by the analysis of recall and relearning scores in the previous section, would have been materially altered if there had been greater or less formal inter-list similarity of the syllables learned. The obtained relationship seems to have been a consequence of the dominance of serial position factors in determining the interference.

Another finding relative to these intrusions must, however, be considered in the formation of such an hypothesis. As shown in Table IX, the locus of the competition between the original and interpolated responses occurs later and later in the relearning process the greater the degree of learning of the interpolated syllables. Three different, though correlated, indices show this effect. As the degree of interpolation increased, the proportion of all the different intrusion syllables occurring on the first relearning (recall) trial decreased from 62.2% to 7.0%,[20] the

[20] In the later theoretical interpretation of these results, an important datum is the absolute amount of RI on the first relearning trial which is attributable to overt intrusions. If it is assumed, following McKinney and McGeoch, *op. cit.*, 420-421,

mean relearning trial during which these intrusions occurred increased from 2.0 to 9.4, and the mean frequency with which the displaced syllables in the original list had been correctly anticipated before their displacement increased from 1.2 to 4.3. These relationships for all intrusions are closely paralleled by those for the intrusions based on identity of serial position, but the relationships in the case of intrusions based on similarity are extremely irregular. Nevertheless, it is clear that for every

TABLE IX

LOCUS OF INTRUSIONS DURING THE RELEARNING PROCESS

	Rest	Trials on interpolated lists			
		5	10	20	40
Number of intrusions					
different, on the first relearning trial	0	28(62.2%)	32(50.0%)	11(29.7%)	3(7.0%)
based on serial position during the first relearning trial	0	17(77.3%)	25(64.1%)	7(38.9%)	1(9.1%)
based on similarity during the first relearning trial	0	2(25.0%)	0	0	2(8.3%)
based on serial position and similarity	0	1(25.0%)	0	0	0
unclassified	0	8(72.7%)	7(77.8%)	4(57.1%)	0
Mean trial of occurrence					
of all intrusions	16.7	2.0	4.7	7.3	9.4
of those based on ser. pos.		1.4	1.9	6.9	12.0
of those based on similarity		3.4	12.7	10.4	8.6
Mean correct anticipations					
of all displaced syllables	8.8	1.2	2.9	2.2	4.3
of those displaced on basis of ser. pos.		1.8	2.4	4.2	8.8
of those displaced on basis of similarity		0.8	6.4	2.9	3.7

that a *correct response* has been blocked by each *different* overt intrusion in the first relearning trial, then the average absolute RI attributable to such intrusions is 0.58, 0.67, 0.23, and 0.06 syllables with 5-, 10-, 20-, and 40-trial interpolations. Since the average total RI in both cycles of the experiment was 2.77, 3.52, 3.83, and 3.44 (Rest — Work) for these RI conditions, the percentages of the obtained RI attributable to different overt intrusions were 20.9, 19.0, 6.0, and 1.8. If, in addition, it is assumed that repeated intrusions block a different correct response each time they occur, these average absolute RI values become 0.75, 0.79, 0.29, and 0.06, and the percentages of the total RI attributable to overt intrusions become 27.1, 22.4, 7.6, and 1.8. These values do not, of course, represent the total amount of RI attributable to overt intrusions, because they refer only to the intrusion of *whole* syllables. Attempts to analyze the data of this experiment for partial intrusions, such as would be involved in the displacement of single letters, have been unsuccessful.

degree of interpolated learning except the highest, the intrusions based on similarity come later in the relearning than the intrusions based on serial position. These relationships serve to emphasize, therefore, another important determiner of the specific relationship between the amount of RI and degree of interpolated learning found in this study; namely, the degree of learning of the original list. In fact, these data warrant the hypothesis that the maximal amount of interference occurs when there is a certain ratio of the degrees of learning of the original and interpolated materials.

This hypothesis cannot be elaborated properly without consideration of

TABLE X

THE FREQUENCY OF OVERT INTRUSIONS OF SYLLABLES FROM THE ORIGINAL LISTS DURING THE LEARNING OF THE INTERPOLATED LISTS
(Both cycles combined)

Experimental condition	Trial during which intrusion occurred				Total intrusions
	1–5	6–10	11–20	21–40	
Continuous learning without prior learning: control (Condition VI)	8	3	7	1	19
5 trials on interpolated list after 5 original trials (Condition II)	14				14
10 trials on interpolated list after 5 original trials (Condition III)	15	9			24
20 trials on interpolated list after 5 original trials (Condition IV)	11	5	0		16
40 trials on interpolated list after 5 original trials (Condition V)	19	5	1	2	27

the frequency of intrusions of original syllables during the interpolated learning. As shown in Table X, the frequency of such intrusions of whole syllables during the first 5 trials was greater under the conditions which involved the prior learning of an original list for 5 trials.[21] For example, 8 such 'intrusions' occurred during the first 5 trials of interpolated learning when there had been no prior learning: whereas, there were 14, 15, 11, and 19 intrusions when the learning followed 5 trials on an original list. All 8 of the 'intrusions' occurring in the control condition were based on the formal similarity of the syllables, and may be attributed

[21] McKinney and McGeoch, *op. cit.*, 413, found that intrusions occurred more often in the delayed recall of the original list than in the immediate recall of the interpolated list. In the present experiment the *Ss* averaged 7.88 different intrusion syllables during relearning and 3.25 intrusion syllables during the interpolated learning. These averages include the intrusions during the 4 work conditions and the 2 cycles of the experiment. The r between the number of intrusions during relearning and the number of intrusions during the interpolated learning was $0.66 \pm .12$.

to normal tendencies to substitute letters or transpose letters in spelling nonsense syllables. In the 4 work conditions the number based on formal similarity was 10, 12, 7, and 17, and the number based on serial position was 4, 3, 6, and 6. Although obviously insufficient for a detailed analysis, it is significant that the frequency of these intrusions is greatest in the first 5 trials of the learning of the interpolated list. The frequency of all proactive inter-list intrusions on trials 1, 2, 3, 4, and 5 was 0, 16, 20, 13, and 10, respectively. With 5 trials on the first list, the specific inter-list interferences occur most frequently in the third trial on a second list learned after a rest of 1 min.

Theoretical Conclusions

From the radically empirical point of view the scientific study of retroactive inhibition may be formulated in the pseudo-equational form RI = f(a,b,c, . . . z), where RI is the amount of loss in retention of an original S-R relationship, X, resulting from the interpolated S-R relationship, Y, and a,b,c, . . . z refer to identifiable aspects or dimensions of the stimuli, the responses, the organism, or the method of measurement employed. Studies involving the systematic variation of the formal, meaningful, or process similarities of the two learning activites, of the degrees of learning of the original and interpolated activities, etc., contribute to the completion of this analysis of the conditions of RI. Clearly, this dimensional analysis is essential to the formulation and validation of a theory of RI. However, some studies contribute directly to the characterization of the behavioral correlates of the measured loss in retention. They cover such problems as the *locus* of the inhibitory processes—whether in the interpolated learning, in the recall and relearning of the original activity, or both, the *direction* of the inhibition—whether the inhibition is mutual or occurs only in the attempt to recall and relearn the original activity, and the *essential nature* of the inhibition—whether it is central or is attributable to the competition between reaction systems. Such observations are sure to play a dominant role in the formulation of a theory of RI, even though the theory must be congruent with the generalizations based on the dimensional analysis of the conditions of RI.

(a) Conditions of retroactive inhibition. It was the intent of the present experiment to hold constant all the known conditions of RI except the degree of learning of the interpolated list of nonsense syllables, in order to determine the relationship between the degree of interpolated learning and the amount of RI when (a) the original list of nonsense syllables was poorly learned; (b) the original and interpolated lists were near the minimum in formal or meaningful similarity, but the

environmental situation and the process of learning—the anticipation method—provided some similarity of the two learning activities; (c) retention of the original list was measured by the recall and relearning methods 30 min. after the end of practice. For this complex of conditions, the data favor the general principle that the amount of RI first increases and then decreases as the degree of learning of the interpolated list is progressively increased. This relationship is clearly present when the retention of the original list is measured by the number of trials required to complete the relearning of the original list to mastery. However, this statement must not be taken to imply a constancy of the relationship under varying conditions.

The form of the relationship depends in part on the measure of retention used. All measures of retention show an increase in RI with early increases in the degree of interpolated learning, but the *decrease* in RI with further increases in the degree of interpolated learning is much less apparent in the recall scores on the first relearning trial than in succeeding relearning trials and in the number of trials required to relearn to mastery. A joint appraisal of these facts leads, however, to a clarifying generalization: RI dissipates when the correct responses are repeatedly reënforced, as is the case during relearning by the anticipation method, and this rate of disappearance of the RI during relearning is at a minimum with intermediate degrees of interpolated learning and at a maximum with low and high degrees of interpolated learning. After 5 trials on an original list of nonsense syllables, the maximal interference with recall (on the first relearning trial) and the maximal persistence of the inhibition during the relearning process occurs with intermediate degrees of interpolated learning, minimal inhibition of recall and minimal persistence of the inhibition occurs with a low degree of interpolated learning, and near-maximal inhibition of recall is combined with *minimal* persistence of the inhibition during relearning when there is a very high degree of interpolated learning.

This conclusion is not at variance with results obtained by McGeoch in an earlier study in which comparable controls and measures were used, although the failure to obtain reliable differences in RI led him to conclude that the intermediate and high degrees of interpolated learning gave 'nearly' equal amounts of RI, however measured.[22] McGeoch's *Ss* learned an original list of 8 nonsense syllables for 11 trials, then learned a second list for 6, 11, 16, 21, or 26 trials, and, after 10 min., relearned the first list to 3 successive perfect trials. In the second cycle the per cents of RI, 100(Rest — Work)/Rest, with the degrees of interpolated learning listed above were 50.1, 89.1, 92.9, 81.2, and *84.4* when measured by the recall on the first relearning trial and 16.6, 50.0, 43.5, 55.0, and *30.6* when measured by the number of trials required to relearn to the stated criterion.

Although the effect of the degree of original learning and the similarity factor on the form of the obtained relationship must await further systematic investigation, suggestions regarding the nature of these interrelationships may be obtained from the data on inter-list intrusional responses in the present study. It is assumed that the inter-list intrusions are responsible, at least in part, for the measured losses in retention.

Some formal similarity of the syllables in the original and interpolated lists was necessarily present in this study, and there were a few instances in which the lists contained syllables that had 2 letters in common. In the analysis of the inter-list

[22] *Op. cit.*, 707.

intrusions it was found that some intrusions were based on these formal similarities, and other intrusions were based on the identity or nearness of the serial positions of the intruding and displaced syllables. Furthermore, the frequencies of these two types of intrusions did not vary concomitantly as the degree of interpolated learning increased. The intrusions based on the identity of serial positions reached a maximal frequency when the interpolated list was learned for 10 trials; whereas, the intrusions based on syllable similarity reached a maximal frequency when the interpolated list was learned for 40 trials. It follows, therefore, that one would expect the maximal total number of intrusions (serial position + syllable similarity) to be obtained with higher degrees of interpolated learning the greater the similarity of the syllables in the two lists. That is, with the degree of original learning used in this study and a greater frequency of similar syllables in the two lists, the maximal frequency of intrusions during recall and relearning, and the maximal RI, would have been obtained with a high degree, rather than an intermediate degree, of interpolated learning. This hypothesis is admittedly crude, but it is suggestive. A precise formulation of it must, of course, take into consideration (a) the possibility, defended in a later section, that the inter-list intrusions during relearning are responsible for only a part of the measured RI, and (b) the fact that intrusions based on the serial position factor occur earlier in the relearning process than the intrusions based on syllable similarity when the degree of interpolated learning is low, and that they occur later in the relearning process than the intrusions based on syllable similarity when the degree of interpolated learning is high.

The obtained form of the relationship between the amount of RI and the degree of interpolated learning is probably a function of the definition of 'degree of learning' and almost certainly a function of the degree of learning of the original list before the interpolation of the second list. McGeoch has surveyed adequately the problems relating to the definition of 'degree of learning,'[23] and in the light of his discussion the present set of generalizations must be made conditional upon a definition of 'degree of learning' in terms of the frequency of repetition of the nonsense syllables and the correlated frequency of correct anticipations. More than this cannot be said until the significance of the other suggested measures of degree of learning has been revealed through special methodological studies.

As for the question whether the same functional relationship between RI and the degree of interpolated learning would hold if the degree of original learning in the present study had been higher or lower, a certain conclusion must again await a direct experimental test. Nevertheless, the data on the frequency and locus of intrusions during relearning offer pertinent evidence on a closely related problem, viz. the relation between the degrees of learning of the intruding and the displaced syllables. As the degree of learning of the interpolated syllables increased there was an increase in the degree of learning of the original syllables displaced by these syllables during relearning. This was especially clear in the case of the intrusions based on identity of serial position. In fact, when the intruding syllable had been very well learned during the interpolated work period the intrusion not infrequently occurred very late in the relearning of the original list and displaced a syllable that had been correctly anticipated a number of times *during the immediately preceding relearning trials.*

[23] *Ibid.*, 704-706.

These data suggest that the portion of the RI attributable to the competition of original and interpolated responses during recall and relearning is at a maximum when the degrees of learning of the competing responses are approximately equal. This hypothesis has received an earlier statement by Siipola and Israel as a result of their analysis of proactive inhibition in code learning and their reanalysis of the data obtained by McGeoch in serial verbal learning.[24] The only evidence against this hypothesis is the fact that the degree of learning of the syllables displaced by *similar* syllables failed to show a positive correlation with the degree of learning of the intruding syllables. Even so, the hypothesis is validated when the two types of intrusions are considered together. There is, therefore, sufficient evidence to warrant the prediction that, under the conditions of the present experiment, the degree of interpolated learning that produces maximal RI will be higher the higher the degree of learning of the original list.

There are undoubtedly other conditions in the present study which determined the obtained form of the relationship between degree of interpolated learning and RI. Such is the basic assumption in any dimensional analysis of the conditions of a phenomenon. However, none of the data of the experiment are relevant even to the formulation of hypotheses concerning these other conditions.

(b) Essential nature and locus of retroactive inhibition. As previously indicated, the dimensional analysis of the conditions of RI serves to suggest the phenomenal characteristics of the inhibition and to test the validity of hypotheses regarding those phenomenal characteristics. The most thoroughly documented theory of RI has emerged from the long series of dimensional analyses conducted by McGeoch. On the basis of these studies he has proposed an identification of the phenomena of RI and of reproductive inhibition.[25] Reproductive inhibition is defined as the decrement in the retention of an original learned stimulus-response sequence when one term of the pair has, between learning and the test of retention, been connected with another and different term. The identification of RI as reproductive inhibition squarely affirms, therefore, the dependence of RI on the identity of either the stimulus or response terms of the original and interpolated learning, and is a specific transfer theory of RI.

McGeoch's transfer theory is clearly favored by the observations of overt transfer of original and interpolated syllables in the present experiment. Similar behavioral data have been reported by McKinney and McGeoch and by Johnson,[26] but these

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[24] E. M. Siipola and H. E. Israel, Habit-interference as dependent upon stage of training, this JOURNAL, 45, 1933, 205-227. The McGeoch studies are those previously referred to, this JOURNAL, 41, 1929, 252-262; 44, 1932, 695-708.

[25] Cf. J. A. McGeoch and G. O. McGeoch, Studies in retroactive inhibition. VI. The influence of relative serial positions of interpolated synonyms, *J. Exper. Psychol.*, 19, 1936, 1-23; J. A. McGeoch, Studies in retroactive inhibition. VII. Retroactive inhibition as a function of the length and frequency of presentation of the interpolated lists, *ibid.*, 19, 1936, 674-693; J. A. McGeoch and Fred McKinney, Studies in retroactive inhibition. VIII. The influence of relative order of presentation of original and interpolated paired associates, *ibid.*, 20, 1937, 60-83; and J. A. McGeoch, Fred McKinney, and H. N. Peters, Studies in retroactive inhibition. IX. Retroactive inhibition, reproductive inhibition and reminiscence, *ibid.*, 20, 1937, 131-143.

[26] Cf. McKinney and McGeoch, *op. cit.*, and L. M. Johnson, Similarity of meaning as a factor in retroactive inhibition, *J. Gen. Psychol.*, 9, 1933, 377-389.

earlier reports were restricted to observations of overt inter-list intrusions during written recalls; whereas, the use of the anticipation method in the present study permitted observations of the locus of these intrusions during the entire relearning process. Furthermore, it has been clearly shown that the frequency of such intrusions during recall and relearning is correlated with the amount of RI as traditionally measured, and that the intrusions may in most instances be traced to one of two forms of identity of the intruding and displaced syllables, namely, an identity of serial position or a partial identity of the letters in the two syllables. Such direct evidence that overt transfer of responses is the behavioral correlate of the measured RI forces acceptance of a transfer theory of RI which identifies at least a portion of the RI with reproductive inhibition.

It does not necessarily follow, however, as McGeoch and McKinney and McGeoch maintain,[27] that all the measured RI may be attributed to competition of the original and interpolated responses at the time of the attempted recall and relearning of the original material. Obviously, the theory of RI must provide an interpretation of any marked discrepancy between the total amount of RI and the amount directly attributable to overt intrusions of interpolated responses. Thus, in the present study, on the first relearning trial after 10 and 40 interpolated trials the percentages of RI were 78 and 76 (averages of both cycles); whereas, the percentages of RI attributable to the intrusions of whole interpolated syllables were 14.8 and 1.8, and the RI persisted throughout the relearning in the first case but disappeared after a few relearning trials in the second case. Some factor or factors other than the overt intrusion of specific responses must be postulated to account for these differences McKinney and McGeoch suggest that the residual RI in recall may be attributed to subliminal or implicit intrusions. Although this interpretation has the advantage that it avoids the postulation of a different form of inhibition, another interpretation is possible. It may be that there is a direct weakening of the original responses at the time the interpolated responses are being learned, and that this unlearning factor accounts for the discrepancies noted.

The residual RI which must be accounted for by postulating one or more factors other than overt specific intrusions increases in amount as the degree of interpolated learning increases. In Fig. 3 the uppermost curve shows the obtained relationship between the total absolute RI (Rest — Work) on the first learning trial and the degree of interpolated learning, and the curve labeled "Overt Competition" shows the relationship between the absolute RI attributable to overt intrusions and the degree of interpolated learning. Since only the intrusions of whole syllables were identifiable in the recall records, and there were undoubtedly many partial intrusions, *e.g.* 2- and 3-letter responses made up of parts of different interpolated syllables, the points on this curve have been fixed by multiplying the percentages of RI attributable to overt intrusions of whole syllables by 2.[28] If it is then assumed that the RI

[27] Cf. J. A. McGeoch, Studies in retroactive inhibition. I. The temporal course of the inhibitory effects of interpolated learning, *ibid.*, 9, 1933, 24-41; Studies in retroactive inhibition. II. Relationships between temporal point of interpolation, length of interval, and amount of retroactive inhibition, *ibid.*, 9, 1933, 44-55; McGeoch and McGeoch, *op. cit.*; and McKinney and McGeoch, *op. cit.*

[28] It is assumed that a correct response was blocked each time an entire syllable from the interpolated list occurred as a response during the first relearning trial on the original list. This total includes repetitions of the intrusion syllables, and is

attributable to overt intrusions combines additively with the RI attributable to the unknown factor, X, the relationship between the latter factor and the degree of interpolated learning is shown in the curve labeled "Factor X." This factor was almost solely responsible for the RI in the first relearning trial after 40 interpolated trials, and this RI has been shown to be extremely transitory. Therefore, the unknown factor must be one that increases progressively as the degree of interpolated learning increases, but ceases to interfere with the recall of the original syllables after they have been reënforced 3 or 4 times.

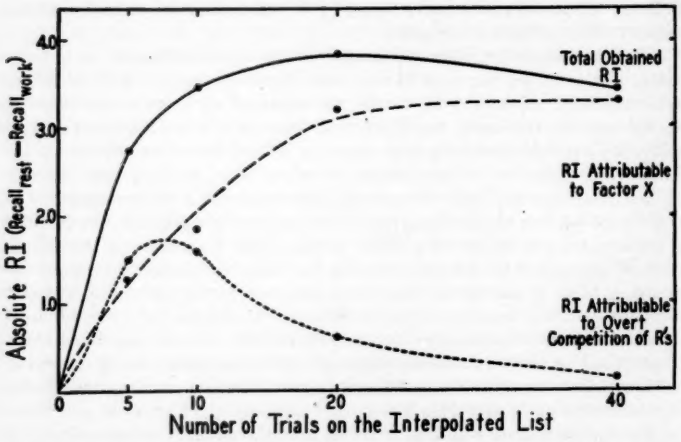

FIG. 3. RELATIONSHIP BETWEEN THE AMOUNT OF RI AND THE DEGREE OF LEARNING OF THE INTERPOLATED MATERIAL

The total obtained RI is the number of syllables forgotten as a consequence of the interpolated learning. The RI attributable to the overt competition of original and interpolated responses has been taken as 2 times the average frequency of intrusions of entire interpolated syllables during the recall trial in order to take into account the unidentified overt intrusions of parts of syllables. The curve for Factor X represents the absolute decrement in recall attributable to the factor or factors other than the overt competition of original and interpolated responses.

The primary question becomes the identification of Factor X. The theory of McGeoch and McKinney and McGeoch involves the parsimonious assumption that this factor is merely another form of competition between the original and interpolated responses at the time of recall and relearning; namely, that it is the sub-

greater than the number of *different* intrusion syllables. See footnote 20, page 190, for the derivation of the values used in Fig 3. The use of 2 as the multiplier is arbitrary, and the form of the curve for "Factor X" is a function of the multiplier used. For example, if 4 were used as the multiplier the curve for "Factor X" would be sigmoid in form. The argument for a 2-factor theory of RI is not dependent, however, on the multiplier used; no multiplier would make the curve for the RI attributable to the competition of responses fit the curve for the obtained RI.

liminal and implicit intrusion of interpolated responses. That implicit intrusions produce some of the RI cannot be questioned. In recall by the anticipation method the *S* sometimes starts to say a syllable and then rejects it as incorrect. The identification of the tentative response as incorrect is clearly indicated by expressions of disgust and by direct statements. Since the *S* may not have time to spell out the correct syllable before the anticipation interval is over, this implicit intrusion effectively blocks the recall of the correct syllable.

Although implicit intrusions undoubtedly occur, there are at present no data regarding their frequency in recall after various amounts of practice on the interpolated responses, and such data are crucial for the evaluation of the McKinney and McGeoch hypothesis. In justice to the hypothesis it should, however, be noted that the frequency of implicit intrusions, relative to the frequency of overt intrusions, should be positively correlated with the availability, in the Thorndikian sense,[29] of the intruding responses, and that this availability of response should increase as the degree of learning increases. Furthermore, the rapid disappearance during relearning of the inhibition attributable to the implicit intrusions is a reasonable expectation. Presumably, both the overt and implicit intrusions have the strength of their association with the current stimuli lessened by their "punished" occurrence in the early relearning trials, and the weakening effect of a single "punished" occurrence should be greater the greater the availability of the response.[30] Nevertheless, the number of such supporting hypotheses necessary for the defence of the major hypothesis indicates clearly the need for direct experimental tests, rather than a further logical elaboration, of the hypothesis.

Available alternative hypotheses regarding Factor X identify it with some process which occurs during the learning of the interpolated responses and weakens the original S-R relationships. McGeoch has argued against such hypotheses,[31] especially the perseveration hypothesis, on the basis of evidence of a relative (percentage) constancy of the degree of RI over intervals varying from 20 min. to 1 week. The thesis is that the percentage of RI should increase with an increase in the interval before recall if a direct weakening of the original S-R relationships occurs during the interpolated learning, because such weakened connections should show the more rapid forgetting characteristic of weak associations when they are subjected to normal intercurrent activities. The failure of realization of this deduction may not, however, be considered conclusive because (a) all the factors in the experiment, particularly the factor of degree of learning of the original and interpolated materials, could not be accurately evaluated, and (b) an arbitrary zero-point was used in computing the percentages of RI. It should be added that the deduction stated above does not necessarily follow from a 2-factor theory of RI which considers only one of the factors to be a weakening of the original S-R relationships at the time of the interpolated learning. At any rate, there are at present no data which definitely prohibit the hypothetical localization of Factor X in the interpolated learning period.

The perseveration theory of Müller and Pilzecker, recently defended by Skaggs, would identify Factor X with a disruption of the normal perseveration of the neural

[29] E. L. Thorndike, *The Fundamentals of Learning*, 1932, 345-352.

[30] This is most clearly suggested by the study of R. E. P. Youtz, The weakening of word-number connections by "punishment," *Psychol. Bull.*, 35, 1938, 690-691.

[31] McGeoch, *op. cit.*, *J. Gen. Psychol.*, 9, 1933, 24-41.

processes involved in the original learning.[82] This theory lacks precise statement and an attempt to evaluate it in the light of particular data usually ends in a theoretical impasse in which one endows neural perseveration with all the properties implied by the results. It is sufficient in the present instance to suggest that the perseveration theory, although perhaps consonant with the negatively accelerated relationship between Factor X and the degree of interpolated learning, would have to be supported by additional postulates in order to explain the transitoriness of the RI attributable to Factor X.

The "disruption hypothesis" of Webb, recently defended by Britt, and the "generalization-conditioned inhibition" hypothesis of James and Gibson are attempts to attribute at least a portion of the measured RI to inhibitory processes localized in the interpolated learning period.[83] Both hypotheses are consonant with a transfer theory of RI since they assume that the original responses transfer (generalize) to the interpolated activity as a result of the similarity of the stimulus or response terms of the two activities. Both likewise assume that this transfer of responses causes the associations between the transferred responses and the stimuli involved in the original learning to be weakened. The difference between these hypotheses is to be found in the assumptions regarding the cause for the weakening of the original S-R relationships. In the "disruption hypothesis" it is said to result from the incorporation of the original responses in the interpolated activity and their consequent dissociation from the original stimulus factors. In the "generalization-conditioned inhibition" hypothesis the weakening is attributed to the unreënforced or 'punished' occurrence of the original responses during the learning of the interpolated responses. Britt's recent elaboration of the "disruption hypothesis" suggests that this difference is chiefly a matter of terminology. A precise formulation of each hypothesis, with due regard for the known conditions of the presence and amount of RI, is lacking.

In lieu of a precise formulation of these theories at this time, it is sufficient to point to the implications of the data of the present experiment. Unless the observed transfer (generalization) of responses from the original to the interpolated activities, and their subsequent inhibition through non-reënforcement or 'punishment,' is to be given no significance, these data clearly favor a theory that attributes a portion of the RI to a weakening or unlearning of the original S-R relationships during the interpolated learning. Overt intrusions of entire original syllables during the interpolated learning occurred with sufficient frequency, especially during the first 10 trials of the interpolated learning, to warrant the assumption that transfer or generalization of response occurred. The fact that most of the intruding and displaced syllables were similar or were in the same serial position in the two lists

[82] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol., Erganzungsband*, 1, 1900, 1-300; E. B. Skaggs, A discussion of the temporal point of interpolation and degree of retroactive inhibition, *J. Comp. Psychol.*, 16, 1933, 411-414; and The concept of retroactive inhibition, *Psychol. Rev.*, 33, 1926, 237-244.

[83] L. W. Webb, Transfer of training and retroaction: A comparative study, *Psychol. Monog.*, 24, 1917, 1-90; S. H. Britt, Theories of retroactive inhibition, *Psychol. Rev.*, 43, 1936, 207-216; H. E. O. James, The problem of interference, *Brit. J. Psychol.*, 22, 1931, 21-42; E. J. Gibson, Retroactive inhibition as a function of degree of generalization, *Psychol. Bull.*, 35, 1938, 626.

lends support to this interpretation, and the generality of the phenomenon may be inferred from the fact that it has been observed by McKinney and McGeoch in written recalls of interpolated lists after learning by the method of complete presentation.[84] Presumably, there were also many unidentifiable partial overt intrusions and implicit intrusions in both studies.

In order to complete this interpretation it must be assumed that the S-R relationships responsible for these complete and partial intrusions suffered inhibition and weakening when the intrusions occurred. This is justified since it is commonly assumed that an unreënforced or 'punished' response will be weakened, and the intruding responses are clearly 'punished' when the anticipation method is used. The question remains, however, whether this 'unlearning' factor could be expected to have the functional characteristics assigned to Factor X.[85] These functional characteristics present theoretical difficulties. Most of the intrusions of entire syllables occurred during the first 5 trials of the interpolated learning, and their frequency did not increase regularly as the number of trials on the interpolated list increased. These facts are consonant with Hovland's recent discovery that generalized conditioned galvanic responses extinguish very rapidly,[86] but, when it is assumed that the total amount of unlearning of the original syllables is a function of the frequency of such unreënforced or 'punished' intrusions, the curve for the relationship between this unlearning factor and the degree of interpolated learning does not coincide with the curve for Factor X in Fig. 3. Furthermore, the rapid dissipation of the inhibition due to Factor X during relearning requires the assumption that the unlearning of the original responses which results from their 'punished' occurrence disappears very rapidly when they are again 'rewarded.' There is no independent evidence on this point either in studies of reward and punishment or in studies of conditioned discrimination.

The identification of Factor X with conditioned inhibition or the weakening of a connection through punishment is not clearly affirmed by the data of the

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[84] *Op. cit.*

[85] It is premature to speculate at this time regarding the nature of this "unlearning process," be it called the weakening of a connection through punishment of it, conditioned inhibition, or experimental extinction. Thorndike, *op. cit.*, 277-313, and G. R. Wendt, An interpretation of inhibition of conditioned reflexes as competition between reaction systems, *Psychol. Rev.*, 43, 1936, 258-281, have suggested that the inhibition or 'unlearning' of one response occurs when it is unreënforced or 'punished' because another incompatible response has been fixated. As Helen Peak and Louise Deese suggest (Experimental extinction of verbal material, *J. Exper. Psychol.*, 20, 1937, 244-261) this incompatible response in the elimination of an available verbal response may be merely the set not to respond. If this interpretation is given to the 'unlearning' factor postulated in the present connection, it is clear that the difference between the 'unlearning' and 'competition' factors is to be found in the nature of the competing response at the time of recall. It might then be maintained that the 'true locus' of the processes responsible for all the RI is at the time of attempted recall and relearning. However valid this contention may be, the separation of the 'unlearning' factor and the 'competition' factor, and the emphasis on the dual locus of the processes responsible for the RI, is a worthwhile theoretical device at the present time.

[86] C. I. Hovland, The generalization of conditioned responses. III. Extinction, spontaneous recovery and disinhibition of generalized responses, *J. Exper. Psychol.*, 21, 1937, 47-62.

present experiment. However, the data are not incompatible with the hypothesis when allowance is made for the fact that partial overt intrusions and implicit intrusions during the interpolated learning were not subject to observation. Neither are they incompatible with the hypothesis which attributes all the measured RI to the competition of original and interpolated responses at the time of attempted recall and relearning. In fact, the data raise many more problems than they solve. Especially do they emphasize the necessity of an experimental analysis of the temporal locus of the factor or factors responsible for the RI. Without question, a systematic study of the initial amounts and the relative persistence of RI with various degrees of original and interpolated learning will be a necessary preliminary to the formulation of a precise theory; and such a study must involve a careful observation and analysis of the behavioral correlates of the RI, such as the inter-list intrusions.

SUMMARY

(1) In the experiment, 24 *S*s learned 1 list of 18 nonsense syllables for 5 trails, rested for 30 min. or learned a second list for 5, 10, 20, or 40 trials, and then relearned the original list to a criterion of 2 successive errorless trials. The anticipation method was used throughout and the erroneous anticipations were recorded verbatim, which permitted an analysis of the relationship between retroactive inhibition and the frequency and locus of overt inter-list response intrusions.

(2) The results support the following generalizations.

(a) All measures show an increase in retroactive inhibition with early increases in the degree of interpolated learning and a *decrease* in retroactive inhibition with very high degrees of interpolated learning. The decrease in retroactive inhibition with high degrees of interpolated learning is much more pronounced in the relearning trials after the first and in the number of trials required to relearn to mastery, which suggests that the retroactive inhibition produced by interpolated learning disappears during relearning more rapidly the greater the degree of interpolated learning.

(b) Syllables from the interpolated list occur frequently as erroneous responses during the recall and relearning of the original list. In general, the frequency of these overt inter-list intrusions is correlated with the amount of retroactive inhibition as revealed by the traditional measures.

(c) The intrusions of interpolated syllables during the recall and relearning of the original list can be attributed to either the similar serial positions of the intruding and displaced syllables or to the identity of letters in the 2 syllables. Intrusions based on the similarity of serial positions occur earlier during the relearning process than intrusions based on the formal similarity of the syllables, and the latter increase in frequency as the degree of interpolated learning is increased; whereas, the former first increase and then decrease in frequency as the degree of interpolated learning is increased. With early increases in the degree of interpolated learning the intrusions based on both factors occur later during the relearning process, but with further increases in the degree of interpolated learning the intrusions based on the formal similarity of the syllables tend to occur earlier in the

relearning process and the intrusions based on serial positions continue to occur later and later during the relearning process.

(d) There are overt signs of proactive inhibition of the learning of the interpolated list. That is, syllables from the original list occur as erroneous intrusions during the learning of the interpolated list. These intrusions occur for the most part during the first 5 trials on the interpolated list.

(3) The following interpretations were ventured.

(a) The results are interpreted as favorable to a transfer theory of retroactive inhibition, as a consequence of the correlation between the frequency of overt intrusions and the amount of retroactive inhibition, and as a consequence of the correlation between the occurrence of an intrusion and the similarity of the serial positions or the formal identity of the intruding and displaced syllables.

(b) The theory of retroactive inhibition which attributes all the inhibition to a competition of orginal and interpolated responses during the recall and relearning of the original responses is, however, questioned. Since it can be shown that there is a large amount of retroactive inhibition on the first relearning (recall) trial after 40 interpolated trials even though there are very few overt intrusions on this trial, it is argued that some factor other than the competition of original and interpolated responses may be responsible for a portion of the retroactive inhibition.

(c) Several theories regarding the nature of this unknown factor are examined, and it is proposed that this factor is a direct 'unlearning' of the original responses at the time the interpolated list is learned. Such 'unlearning' would presumably follow from the unreënforced occurrence of the original syllables as erroneous responses during the learning of the interpolated list, this phenomenon having been demonstrated in the data of this experiment.

GEOGRAPHICAL ORIENTATION

By T. A. RYAN and M. S. RYAN, Cornell University

The topic of orientation with respect to places, directions, and positions has long been of interest to psychologists and to students of biology. The ability of animals to travel long courses over unknown territory, the differences among men in skill for maintaining direction, and those frequent illusions of place, distance, and direction which annoy and baffle the human traveler, have especially been made the subject of discussion, casual description, and occasional experimentation.

The present study concerns an aspect of the general problem of orientation in man somewhat unlike that found in most researches, where emphasis has been laid upon 'objective accuracy' and upon its conditions.[1] The 'objective' approach is of obvious value in a practical way. It commonly fails, however, to inform the reader upon the nature of the psychological processes involved in orienting, *e.g.* how the individual perceives his surroundings as related to himself and how he passes from the immediately perceived field to more remote regions. It is this passing from the immediate field—as, for instance, from this room where I now sit—to objects, scenes, and events lying beyond, including them all in one plan of orientation, which we especially consider in the present experimental study.

In accordance with our aim to investigate the nature of psychological performance involved in geographical orientation, we have undertaken experiments which are descriptive in character. Any precise measurement of 'objective' or physical accuracy of orientation we have deliberately left out of our procedure. Our problem was simply to find out what orientation in a general geographical setting is like *for the observer*. This, as it seems to the writers, is the first question to be asked in any such inquiry where an unknown performance of the organism is to be investigated.

The general plan was to ask *O* a question or to set him a problem concerning geographical relations, and then to obtain a description, as exact as possible, of

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[1] The general literature may be roughly set down under four headings; (1) theoretical discussions, with emphasis upon instinct, (2) personal accounts of illusory orientation, (3) experimental studies of homing among animals, and (4) experiments upon human accuracy in orientation.

the activities by which *O* arrived at his answer.[2] The several series to be described differed in the kind of orientation involved or in the sort of question asked.

The crux of our method lies in making the questions difficult enough or in making the orientation difficult enough to provide a clear cut performance. For in most skilled performances the products are so condensed and foreshortened as to make description extremely difficult. As we engage in everyday activities among familiar surroundings, orientation likewise becomes so stable and foreshortened that we scarcely notice that we *are* oriented, to say nothing of an ability to describe just what the means and the terms of orientation are.

In an early series of experiments we met this difficulty by disturbing the orientation of *O* in the immediate perceptive field, making it more laborious without destroying it altogether. In the later series *O* was comfortably oriented, but the questions which he had to answer were either unusual or else they forced him to go beyond the perceptive surroundings.

Series 1. O was seated in a rotating chair and given the following general instruction:

> This is not an experiment in rotation. You will be blindfolded and moved about very slowly. If you feel vertigo or dizziness at any time please report it at once and the experiment will be stopped.

The more specific observational instructions were as follows:

> At the signal 'ready' you will be moved slowly about. At the signal 'now,' movement will stop, and *E* will ask a question. As soon as you have answered the question, report upon your performance in terms of product. Report everything which occurs from the 'ready' signal until the time of report.

O was rotated slowly by hand, first in one direction and then in the other but in random order. As *E* rotated the chair he also moved about in the room so that his voice would not serve as a fixed point of reference, coming as it did just at the crucial time when movement ceased and *O* was interrogated.

The observations took place during the morning when accessory sounds were relatively unobtrusive. Since the *O*s were not to be completely disoriented and since we were not studying the conditions which govern spatial placing, random noises were to our advantage rather than disturbing factors. The only thing which we wished to avoid was a constant cue which would remain the same from observation to observation. This would lead to rapid habituation, which we were trying to avoid.

After *O* had reported, he was instructed:

> Remove the blindfold and report what occurs as you do so.

This latter instruction was added with special reference to those cases where the blindfold orientation was objectively disturbed. We wished to obtain a description of re-orientation when visual pattern was restored.

The tasks for *O* in this series were of two general kinds; (a) pointing toward objects within the room, and (b) pointing in the direction of invisible objects outside (usually buildings on or near the campus).

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[2] The following *O*s, all practiced in observation, served in the experiments: Dr. Cleo Chrisof (*Ch*), Dr. R. M. Lindner (*Li*), F. N. Jones (*Jo*), J. I. Lacey (*La*), and J. W. Macmillan (*Ma*).

Series 2. This series was made up of several groups of questions and problems. *O* was seated comfortably in a familiar room of the laboratory and given the following instructions:

I will ask you a question. After you have answered it, report everything which has occurred from the time of the question until your answer.

The tasks presented were of four kinds: (i) *O* was asked to point in the direction of some city or town. (The list included cities in near-by regions and also places at greater distances.) (ii) Tasks took the form of giving the campass directions from Ithaca to some other place. (iii) The task was the same except that *O*'s home city or town was substituted for Ithaca. (iv) *O* was presented with questions in the form: "Give the compass directions from —— to ——." Pairs of cities from all parts of the world were used.

Ten tasks of each kind were set for each *O* (40 questions for each of 5 *O*s). Because of the fulness of most of the reports, only 3 to 5 reports were secured in each observational period.

The conclusions and results of this study are then based upon a total of 260 separate descriptions, secured from 5 trained *O*s.

Experimental Results

The reports show almost without exception that the act of orienting occurs in several successive stages. Before considering the whole process, therefore, it will be well to consider the nature of the stages in the procedure, and later their integration and the transition from one to another. These momentary orientations may be considered as the *materials* which the organism makes use of in working out the task. Of these there are several main groups, as shown in Table I.

TABLE I

Kinds of Orientation

(A) Immediate perceptive orientation
(B) Relation of perceptive field to other objects outside of the field (primary directionalization)
  (1) specific objects related directly to perceptive field
  (2) primary directionalization in compass coördinates
(C) Orientation deduced from (or held in) verbal formula
(D) Directional relations between objects outside the perceptive field
  (1) maps and 'air-views'
  (2) telescoped imaginal traveling

The primary orientation within the immediate perceptive field will be taken for granted. This is not to say that perceptive orientation is not an important psychological problem, nor that it is not important in the wider kind of orientation in which our interest lies. But it is in itself such a wide topic that we must be content to refer the reader to such

summaries as that of Gibson.[3] For purposes of the present discussion, we shall take orientation in the directly apprehended field as one of the 'givens' from which we set out, noting that it may take various forms. We center our interest in those moments where the orientation of the organism gets away from the structure of the immediate field to objects, places, and directions outside the perceived 'here and now.'

Primary directionalization. One of the simplest and commonest, and at the same time one of the most difficult to understand from the point of view of ordinary accounts of psychological activity, is what we shall call 'primary directionalization.' Here the relation of the 'here and now' to other places is inherent in the present apprehended scene.

The commonest mode consists in a direct *implication* by objects in the field of something which lies just-beyond. This was particularly common in our rotational experiments. For example, *O* reported that he went around in a visual place even though blindfolded. Rotation was accompanied by visual 'imagining' of the objects in the room. We cannot say that one 'imagines' the rotation, for the visualizing was really an integral part of the perceiving. The translation of the body as perceived directly *implied* corresponding changes in the visual scene.

The same sort of implication was carried still further when *O* was asked to localize objects outside the room. The south wall of the room seemed to imply directly the library which lay beyond. One of the characteristics of the south wall was that it was in the direction of the library. The question did not necessarily do anything except to bring out this characteristic of objects already present.

In our later experiments we found primary directionalization of this kind carried still farther (farther in the literal, geographical sense, not necessarily in the sense of a more complicated performance). The fact that 'New York is over that way' seems to have become an immediate characteristic of a certain portion of the visual field. When asked to point toward a distant but familiar place, *O* behaves as though it were part of his immediate surroundings or at least immediately bound up with some aspect of the present perceived scene.

We shall select a few examples from the large number found in the reports:

Vis. per (room going around faster than I was moving). [It changed in rate, but it was hard to keep it going as slow as I perceived myself as going.] (*Jo*-1-2)[4]

In this instance the visual accompaniment is, as often occurs, 'deliberately' controlled by *O* for the purpose of making his task easier. But at the same time we notice that this visual pattern has a more or less independent existence, and in many

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[3] J. J. Gibson and O. H. Mowrer, Determinants of the perceived vertical and horizontal, *Psychol. Rev.*, 45, 1938, 300-323.

[4] The letters in parentheses indicate the *O*, the number of the series, and the number of the item in the series, in that order. Common abbreviations which occur in recording the descriptions are as follows: *per* for perceiving, *imag* for imagining, *com* for comment, *rem* for remembering, *insp* for inspecting. Items in parentheses are the products of the performance, *i.e.* a description of the things perceived, imagined, etc. The sign > means "successive functional phase."

We must emphasize that descriptions like the above are excerpts and simply describe one stage of a longer process.

of our examples we find that the visual scene is 'just there' much as it is in ordinary perception. In the following instance this is the state of affairs:

Imag (room: to determine my position). [*O* questioned concerning this statement.] > per (self as sitting facing the door into the other room). [I was just as sure of it as if I had my eyes open.] (*Ch*-1-5)

The reports indicate that the process is not to be considered as somatic perception *plus* the auditory perception *plus* visual imagining, but that the whole process is organized and turns out a more or less unitary field. In fact, there seems to be an ever increasing body of evidence that space as perceived, especially bodily space, is not to be separated into compartments such as tactual space, visual space, auditory space, and so on. Even before the beginning of the century Washburn pointed out that vision plays an integral part in localization on the skin—that visual imagery is not just a by-product, but that it actually contributes to the perception.[5]

Perceived objects in space may be non-specific as far as sensory modality is concerned (Galli).[6] Successive visual and tactual stimuli led to reports of 'something moving' from one place to another, the 'something' being neither visual nor tactual.

Again, tactually perceived things may extend beyond the limits of the body, as Hoisington's experiments upon the apprehended length of lifted rods demonstrated. Here it was found that the perception of length "is based primarily upon the experience of two opposed pressures whose intensive ratio falls within certain limits."[7] Our concern here is not, however, with the essential factors underlying such perception, but with the nature of the perceived result. The fact that Hoisington has to resort to exact control of conditions in order to demonstrate that specific factor is in itself sufficient evidence that we do not ordinarily perceive *pressures* but a qualification (length) of the object.

In Hoisington's experiment, visual 'imagery' seems to play a minor rôle. Perhaps this is because the underlying factors are so specific for the perception of length that vision was unnecessary. At any rate, in the general kind of perceptive orientation here discussed the factors are not very specific. This non-specificity may explain why the perceiving of distant objects is visual rather than directly tactual, as in Hoisington's 'length.'

Putting these varied facts together, it would seem best to discount the term 'imagining' which was used by our *O*s to describe the visual aspects of their orientation with respect to near-by surroundings. The visual pattern which they describe is a closely integrated part of the perceived orientation. *O*s label their performance 'imagining' merely because visual receptors are eliminated by the blindfold, but *descriptively* the visual patterns are perceptive in character, forming an extension of tne space which is directly attributable to stimulation.[8]

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[5] M. F. Washburn, Über den Einfluss von Gesichtsassociationen auf die Raumwahrnehmung der Haut, *Phil. Stud.*, 2, 1895, 190-225.

[6] P. A. Galli, Über mittelst verschiedener Sinnesreize erweckte Wahrnehmung von Scheinbewegungen, *Arch. f. d. ges. Psychol.*, 85, 1932, 137-180.

[7] L. B. Hoisington, On the non-visual perception of the length of lifted rods, this JOURNAL, 31, 1920, 146.

[8] We did not question *O* concerning functional labels, since he was instructed to emphasize the description of functional products. The important thing for us here is the recognition of the close relation between these visual patterns and other perceptual patterns in the total orientation at any moment. Whether we define

Primary directionalization with respect to more distant objects. Many of the orientations which fall here are essentially like those already illustrated. *O* is not only oriented to objects in the room, but also to near-by buildings and landmarks. Here perceiving follows the same pattern as in rotation. Consequently we shall illustrate here only those kinds of relation to distant objects which show a broader sweep of orientation. The greater geographical range may imply only greater background and previous training, with no evidence of a more complicated performance.

One difference, however, does appear. The field becomes less unitary in the sense that the objects toward which *O* is oriented are not within a single perceived area. Objects which are in the field *imply* the more distant objects. This implication is extremely difficult to describe. A characteristic of a particular street or road is that it takes you to the place in question (*e.g.* "this is the Elmira road"). This is more than a mere verbal label. Whither the road leads has become a property of the road itself, analogous to the *Aufforderungscharakter* of the objects involved in acting.

At the same time, however, more is involved than this implication-character of present objects. The distant town or place is somehow *present,* although out of sight, in much the same way that the objects of the room in the first series were present although not visible in the ordinary sense of the word.[9] The distant town is present in the sense that it exists in the vague boundaries of the field, but it does not come out of a separate act of imagining.

The following examples illustrate the orientation with respect to distant objects with which we are now concerned:

(1) Per ("Greenland") OI (determine its position) > imag and insp (Greenland is behind me) [automatic pointing]. (*Ch.*-2-ib-4)
(2) Imag (Porto Rico in front of me and to the left). (*Ch.*-2-ib-8)
(3) Per plus comp (question) >search [automatic pointing] >com ("that way") >vis imag (hazy outlines in that direction). (*Jo*-2-ia-2)
(4) Com ("Elmira over that way") >imag (inlet, bridge, hills, road in valley) > comp (at end of this road is Elmira). (*Li*-2-ia-1)
(5) Insp (own position) >imag (wall cut out so I could see Library) >insp (London lies that way and a little this way). (*Li*-2-ib-1)

(Two *Os* almost never reported primary directionalizations of this kind, substituting modes of orientation to be discussed later.)

These descriptions are almost always vague. *O* lacks words for adequate description. Yet when we put all of the instances which bear a general resemblance together, the features which we have describd come out more clearly. It may be that in our introductory remarks about this kind of directionalization we have been too explicit and concrete. But this was done in order to catch its essence. It is by its nature very difficult to set down on paper in a coherent way.

Let us summarize. We have illustrated two closely related kinds of report. In the first, the immediate perceptive field is filled out and extended to the just-beyond in space where the observer *sees* things which are not physically present. Yet he sees them in such a way that

'imagining' in such a way as to include or exclude such patterns is a matter of convenience as far as our present problem is concerned.

[9] For an illustration of this 'non-qualitative' presence see K. Koffka, *Principles of Gestalt Psychology,* 1935, 322.

they form part of the total spatial framework in which he apprehends himself and in which he is orientated. Some would call this 'imagining,' but it is rather a form of perceiving.

In the second kind of report, objects at a relatively great distance do not perceptively become a part of the general field. Yet they are present. They are 'off there' and out of sight, and yet 'really there.' Objects in the present field imply and point toward them. The observer is still oriented with respect to these distant objects as he is oriented to the window which is behind him but which he 'knows' to be there.

Primary directionalization with respect to compass directions. For certain *Os* the present perceptive field frequently has an inherent property of 'compass' direction, for one of our *Os* the compass directions are almost always something which must be worked out in words and step by step. The first sort we consider first. Here a street is at once a way-with-trees-down-the-side and a north-and-south-way. This without verbal label or phrase 'associated with' the object.

We might say that the organism has built up a more or less permanent compass-framework into which is fitted the present scene. But the notion of framework is itself in need for further clarification. We have no evidence that the 'framework' exists for the individual in any psychological manner. The term is just a convenient abstraction which describes a characteristic inherent in many concrete perceptual situations. We may, however, use it as a convenience.[10]

Before considering our illustrations of this primary directionalization of the compass variety, let us mention one other way in which it may be treated. Some may contend that the compass orientation is the result of an implicit generalized muscular response; that, when *O* is operating under a task to find his way or to place distant objects, the old boy-scout method of finding directions by reference to the right and left hands and the like has become foreshortened to incipient pointing gestures which give a cue to direction. This is a definite possibility, and as a matter of fact many of the examples we shall cite involve incipient, as well as overt, pointing. The objection which we may raise regarding this as a solution of the problem is simply that muscular gestures are not directions, even though they may be indispensable to orientation. A description of muscular twitches cannot substitute for a description of the psychological performances and objects. The fact remains (and will be illustrated in the following descriptions) that direction *is a property of the scene* remembered, perceived, or imagined.

We turn to reports.

(1) Search plus imag (a kind of bodily posture: myself going south with Baltimore as goal). (*Ch.*-2-ia-3)
(2) Insp (I'm facing west). (*Li*-2-ia-2)

[10] Frameworks may, to be sure, become, on occasion, separate objects as the result of more complex activities, as was suggested in the preliminary paper on this subject: T. A. Ryan, Dynamic, physiognomic, and other neglected properties of perceived objects; a new approach to comprehending, this JOURNAL, 51, 1938, 641. It is not necessary, however, to call upon such a notion in describing the sort of orientation under consideration at the moment.

The description in these cases seems to be more meager than it is in connection with any of our other categories. But the meagerness of the descriptions is itself an indication that the directionalization is inherent in the field. Otherwise *O* would have to go through a more complicated performance to arrive at it.

The following illustrations seem to indicate that even where *O* was directly oriented with regard to the distant object this compass direction was inherent in the general orientation with respect to the distant object. In other words, the field involved both direct implication of the distant object and at the same time directionalization with regard to compass directions.

(3) [Automatic pointing behind: translation into] com ("north"). (*Cb*-2-ib-6)
(4) Imag plus insp (Rome in the direction behind and to the left of me) [checked movement of pointing in that direction by reinstatement of] > FI > com ("southeast"). (*Cb*-2-ib-7)

We shall come back to compass directionalization in connection with some of our later kinds of orientation, for it is naturally intimately concerned in many sorts of performances.

Orientation deduced from verbal formula. Under this category we mean to include those instances in which *O* is unable to solve the problem by means of primary directionalization of the field, and is forced to proceed by a series of deductive steps in which verbal patterns play a dominant part.

In *O*s who rarely exhibit primary directionalization of the compass variety, the orientation is not usually a property of the perceptive field in the implicit way described above. Rather is orientation secured by means of labels of a purely verbal sort which are associated with certain familiar objects. For example, there is a hill visible from the windows of the laboratory which is named "West Hill." This often served as an explicit point of reference for deducing orientation. As a rule, bodily movements, postures, and gestures were coupled with the verbal label.

In terms of the genesis of orientation in the individual, such verbal deduction may represent an early stage out of which some *O*s have never progressed. If this is so, the problem of genesis is difficult because we have to explain how the organism progresses from verbal labels to new properties of the perceptual field itself. Again, however, we cannot carry this further because of our principal interest in description.

Examples of purely verbal deduction of orientation follow:

(1) Insp (which direction am I facing?) > imag plus insp (surroundings) > com ("lake is on right-hand side," "lake is west," "west is to the right"). (*Cb*-2-ia-1)

Note that even here there was primary directionalization of a simple kind. The lake was not visible to *O*, who 'just knew' it was to her right; but verbal deduction was needed for the compass directions.

Once orientation was thus determined, it was carried over in verbal but foreshortened form from one task to another, as the following quotation from the next report shows:

(2) Insp (where am I facing?) > rem (discovery in the last problem that I am facing south). (*Cb*-2-ia-2)

In other instances, even where *O* was completely directionalized in his surroundings, distant objects were placed in a purely verbal way. For example, Virginia is understood as a southern state without being actually related to the present perceptual field. *O* of course 'knows' this even when he is completely disoriented in the present perceptive field. Even for unfamiliar places, a few remembered verbal tags or automatized verbal associations may come into the solution of the problem. The distant place is a topic or concept completely unrelated to the surrounding scene. The following examples have been selected to illustrate this sort of understanding of a place name.

(1) Comp (Europe is north of U.S.) > comp (adjusting pointing to take account of this). (*Jo*-2-ia-9)
(2) SI (remember that Tokyo lies farther north than most people think). (*Jo*-2-ia-10)

Directional relations outside the perceived field. Some of the tasks set could be solved quite independently of orientation or lack of orientation within the present scene. In these cases, *O* resorted to imaginal devices of various kinds. Some of these are maps and 'airplane' views to be expected, others were less obvious. In spite of the obvious nature of the maps and air-views, there are certain facts of psychological import which appear in the results.

First, the maps were more than mere pictures: they were tied up in various complex ways with the orientation of *O*'s body. This is especially true when maps were used in solving problems of pointing toward distant cities, or in giving compass directions from *O* toward a distant place. Here obviously the map is of little use unless its direction schema is somehow related to the present scene. The question of how this relation comes about and what it is like is not only important for orientation but also for the problem of symbols and signs at large.

Even where the present situation is not under consideration, *O* is somehow oriented in the imaginal scene with respect to the map, with respect to compass coördinates, and the like. The map seems often to be something which includes *O* in it rather than something passively looked at.

The bird's-eye or airplane view is used in exactly the same manner as the map, and may be employed in familiar or unfamiliar territory.

The important psychological question in both cases is how *O* gets direction from the map. This is usually a highly practiced procedure difficult to divine from the reports. *O* usually speaks of "reading off the directions from the map" and can say little more about it. Apparently the directions have become just as much an inherent part of the map as they are in the perceived scene in primary directionalization. In other words, there are no 'symbols' for direction on the map: lines on the map do not *stand-for* directions. A diagonal from lower left to upper right simply *is* a north-east line, and "north-east" (in the report) simply describes this direction *on the map*.

In those cases where *O* must relate map-directions to orientation of his body in the immediate surroundings (esp. in Ser. 2-ia), we gain a little more insight into the relation of directions on the map to directions in the geographical environment.

Sometimes the imagined map is literally made a part of the immediate surroundings, as in the following instances:

(1) Imag (physical map with myself here and with the ocean and London beyond). (*Li*-2-ib-1)
(2) Imag (whole area around here—lake, Ithaca, Cortland, Elmira, scenery as seen from here and from an airplane) > comp (Elmira was on this map in that direction) > [pointing]. (*Ma*-2-ia-1)

We insert the following description to show the difference between an imagined map completely divorced from the present situation and the above descriptions where the imagined map merely extends the perceptive situation at its borders:

(3) Vis imag (surface representing a map—no particular form—, in one corner Ithaca and in other "New York" spelled out) > com ("I know the direction—New York is SE of Ithaca, but she wants me to point to it. Can it be done?") > com (go down State St., turn to left, etc.) comp (Impossible to point to it, I don't know all the turns). (*La*-2-ia-2)

In a few instances, like (1) and (2) above, the map is explicitly 'tacked on' the immediate surrounding. More frequently, however, there is little more than a very vague 'taking account of' or 'reference-to,' which, for the *O* highly practiced in reading maps, seems to be about all that can be said in the way of description. For example:

(4) Vis imag (dark brown map of U.S. as though I were standing on hill or up in the air looking down towards the Miss. river) [pointing to direction of New Orleans as it was on the map.] > insp (that way and a little that way since I'm here). (*Jo*-2-ia-4)

Note that the imaginal maps used in these pursuits are often more than conventional maps; they are condensed or schematic landscapes.

Telescoped imaginal traveling. This way of identifying places might be called 'temporal mapping,' since it is, like maps and air-views, condensed and foreshortened, but it also involves a series of created events out of which orientation or localization comes. Here *O* divines the direction to a city or the relation between two cities by means of an imaginal trip from the one place to the other. The whole temporal process is very brief and highly schematc and during the devised trip, orientation of the sort described as 'primary directionalization' is involved. Each road over which *O* travels is *directionalized* either with respect to the compass directions or with respect to the coördinates of the present field. From this point of view, then, this imaginal traveling is to be understood as of exactly the same mode as primary directionalization in the perceptive field. There are, however, other aspects to be considered.

In imaginal traveling *O* ignores details of the route, devising a single direction which sums up the whole trip. It is as though *O* were a geometer, adding up all of the angles to find a resultant direction. Actually, of course, this is not done explicitly, and *O* is usually quite unable to state how he arrived at his final result. Apparently remembering or imagining a familiar route involves a reduction to essentials and a loss of irrelevant details, without any necessity for logical deduction.

Following are some examples of this sort of orientation:

(1) Rem (driving through Trumansburg and going in that general direction) . . . insp (*this* way then *that* way from here). (*Li*-2-ib-3)
(2) Rem (road) > imag (going out that way) > imag (turn in road by stadium) > imag (Ithaca here and Dryden there, disregarding the turn) > comp (Cortland east and slightly north). (*Li*-2-ib-6)

(3) Com ("windows of Morrill Hall facing campus are to the left, go straight down College Ave. to State St. turn right and then right again") *cum* kin per (turning trunk) > comp (directly in back). (*La*-2-ia-2)
(4) Imag insp (self driving to Annapolis) > com ("north-east"). (*Ch*-2-ii-2)

Imaginal traveling of this kind appears only twice in *Jo*'s reports. *Ma* reports something like it frequently but it seems to be less explicitly an actual traveling, and more often is a sort of elaborated map with details along the road added.

The total course of performance. Having considered the temporal units which are the parts of an entire solution, the description of the total course of performance can be briefly considered.

Of the rotational experiments little need be said since the whole course was made up of a series of short primary directionalizations in the perceptive field (Table I, A and B). These two kinds of orientation succeeded each other or were variously combined.

In the later experiments, where *O* had to relate his perceptively present orientation to distant cities and places, the total course of the performance was more complex. *O* typically shifted rapidly back and forth from one to another of all of the fractional means of orientation which we have described. Very little in the way of a typical pattern, or order of progression can be made out since the mode varied with the type of problem, with *O*'s knowledge of the places involved, and with *O*'s skill in orienting performance.

It must be said, however, that the 'directional' aspect of the problem tempered everything which *O* did. Thus while the course of performance may seem haphazard and random when considered as momentary devices, this appearance is an artifact of analysis. We must remember that all of the items which we analysed out had in common directional properties which made them more or less interrelated, interchangeable, and fitted to serve similar ends.

Summary and Conclusions

When regarded from the standpoint of psychological description, the world to which the individual geographically orients himself may be shown to include much more than the physical surroundings which immediately affect the organism would imply. First, the perceived field is variously extended outward from its primary boundaries. Secondly, it commonly has incorporated with it such a system of reference as the mariners compass suggests, this system becoming part and parcel of the entire 'present' field, the two moments being as inseparable as color and shape in the object.

In the light of our descriptive survey, it would seem that a related

study by one of us had exaggerated the distinction between 'personal' and 'impersonal' frames of reference. There can be no sharp general line drawn between a 'personal orientation,' in which body-objects and their positions and relations are focal, and an 'impersonal' frame from which body-objects are absent. Indeed, the processes of spatial orienting appear, when reported descriptively, so fluid that any emphasis placed therein upon a scene or object—whether body-object or other object—is, as a rule, only momentary, fragmentary, and passing.

A by-product of this study holds a certain importance for the psychology of comprehending and thinking. One might well expect to find that tasks involving the complicated relations of space and place would fundamentally demand signs, symbols, elaborative inductions, and other non-apprehensive devices commonly associated with higher mental operations. We seem to find, however, that the orienting tasks chiefly call for an extension of the basal perceptive mode, into which imagining is sometimes incorporated in a restricted way.

It is primarly to the constant and skilful exercise of these two apprehensive functions and to the relatively invariant physical conditions of the environment that the adult human organism owes its coherent and stable chart of the earth's surface, to which it actively, constantly, and dextrously orients itself from moment to moment and from year to year.

THE CHRONAXY OF COLD AND WARMTH

By F. Nowell Jones, University of Alabama

Although essentially a physiological measure,[1] chronaxy may be applied in various fields of psychological research. The applications to date have, however, been relatively few in number, and the bulk of them have been in the field of sensation.[2] The chronaxy of various of the sensory end-organs has been measured: for vision,[3] taste,[4] the vestibular mechanism,[5] and the skin senses. The first experimental work upon the sensory mechanisms of the skin was done by Bourguignon, who stimulated the sensory nerves directly.[6] Stein,[7] Ruffin,[8] and Walthard and Weber[9] stimulated the

* Accepted for publication June 15, 1939. This study was made in the Psychological Laboratory of Cornell University under the direction of Professor Karl M. Dallenbach.

[1] Louis Lapicque, *L'excitabilité en fonction du temps,* 1926, 1-371. Henri Fredericq, Chronaxie, *Physiol. Rev.,* 8, 1928, 501-544. M. O. Wilson, Chronaxie, *Psychol. Bull.,* 32, 1935, 4-32.

[2] The experiments of Jasper upon handedness (H. H. Jasper and Marthe Bonvallet, Rôle de l'écorce dans l'organisation asymétrique des chronaxies des nerfs symétriques chez les rats droitiers ou gauchers, *C. R. Soc. Biol.,* 116, 1934, 991-994; H. H. Jasper and E. T. Raney, The physiology of lateral cerebral dominance, *Psychol. Bull.,* 34, 1937, 151-165), of Kreezer and Bradway upon intelligence (George Kreezer and K. P. Bradway, Relation between Binet mental age and motor chronaxia, *Arch. Neurol. & Psychiat.,* 34, 1935, 1149-1171), and of Bourguignon upon emotionality (G. Bourguignon, Relations de la chronaxie vestibulaire avec l'émotivité à l'état normal et dans pseudo-mongolisme et la demence précoce, *C.R. Acad. Sci.,* 193, 1931, 250-253), may be cited as examples of experiments in other fields. In these investigations, the attempt was made to find some relation between certain chronaxy values and some trait of the subjects. Thus Jasper reported a particular pattern of extensor-flexor muscular chronaxy in the right-handed individual, which was reversed in the left-handed subject. Kreezer and Bradway found a statistically significant difference between the average chronaxy of adult feebleminded *Ss* with a mental age of 3 yrs. and that of adult normal *Ss*, for the motor point of the triceps, but not for two flexor motor points examined, for other chronaxic indexes, or for higher mental ages in the feebleminded group. Bourguignon claims that emotionality and vestibular chronaxy go hand in hand, but the reliability of his ratings of emotionality is in doubt. For this type of problem, then, chronaxy may or may not be useful, dependng entirely upon the particular question to be answered.

[3] G. Bourguignon and R. Dejean, Double chronaxie du système optique de l'homme, *C. R. Soc. Biol.,* 94, 1926, 750. S. L. Last and F. Laubenthal, Beitrag zur normalen und pathologischen Chronaxie des optischen Systems, *Dsch. Zsch. f. Nervenhlk.,* 129, 1933, 278-282.

[4] A. Chauchard and B. Chauchard, Recherches quantitatives sur l'excitabilité de l'appareil du goût chez l'homme, *C. R. Acad. Sci.,* 184, 1927, 1273.

[5] G. Bourguignon and R. Dejean, Chronaxie normale du nerf vestibulaire de l'homme, *C. R. Soc. Biol.,* 94, 1926, 750.

[6] G. Bourguignon, *La chronaxie chez l'homme,* 1923, 1-417.

[7] H. Stein, Untersuchungen über die Chronaxie der Sensibilität, *Dsch. Zsch. f. Nervenhlk.,* 100, 1927, 221-232.

end-organs in the skin. Stein and Ruffin failed to indicate the modality with which they worked. This criticism applies only in part to the work of Walthard and Weber. Working with pressure and pain, Walthard and Weber differentiated the quality of the sensation at the time the rheobases were established, but neglected to do so when the chronaxy masurements were made. Neff and Dallenbach were the first experimenters to insist upon an identity of quality for rheobase and chronaxy, and they have reported values for pain and pressure which meet that condition.[10] Subsequently Opitz[11] and Hut[12] have also reported work upon pain and pressure which follows the procedure of Neff and Dallenbach.[13]

In the case of the temperature senses, the only worker reporting a successful chronaxy measurement is Bourguignon, who gives the chronaxy of warmth as being about 10 times that of the motor nerve in a given area.[14] He states explicitly that cold cannot be aroused by electrical stimulation. Hut also reports failure in his attempt to determine the chronaxy of cold.[15] Unpublished attempts in the Columbia and Cornell laboratories to determine the chronaxies of the temperature senses have proved unsuccessful both for cold and for warmth.

In the field of the skin senses, then, the measurement of the chronaxy for cold, and the verification of Bourguignon's determinations for warmth, still remain to be done. Here, then, is the purpose of the present study. We wished to discover whether we could under any conditions measure the chronaxies of the temperature senses.

Preliminary Experiments

We anticipated, because of the previous failures and the masking effects of pain and pressure, that the chronaxy measurements of cold and warmth would be difficult.

[9] H. Ruffin, Chronaximetrische Untersuchungen des sensiblen und optischen Apparates, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 140, 1932, 641-656.

[9] K. M. Walthard and A. Weber, Zur Chronaximetrie der normalen Hautsensibilität des Menschen, *ibid.*, 67-88.

[10] W. S. Neff and K. M. Dallenbach, The chronaxy of pressure and pain, this JOURNAL, 48, 1936, 632-637.

[11] G. Opitz, Untersuchungen über die Hautsensibilität. 5. Über die Chronaxie der Schmerzreizung, *Arch. f. d. ges. Physiol.*, 239, 1938, 736-747.

[12] L. J. Hut, *De sensibele chronaxie*, 1936, 88.

[13] Cf. Emil von Skramlik, Beiträge zur Psychophysiologie der Sinnesleistungen: Psychophysiologie der Tastsinne, *Arch. f. d. ges. Psychol.*, Ergänzungsband, 4, 1937, 1-510, 408, for a discussion of the necessity for following Neff and Dallenbach's suggestion that the rheobase and chronaxy qualities be the same.

[14] G. Bourguignon, Chronaxies normales des terminaisons servitires cutanées chez l'homme, *Amer. J. Physiol.*, 90, 1929, 294; Électrophysiologie. Interprétation des sensibilités thermique et douloureuses à l'aide des chronaxies sensitives cutanées normales et de leurs variations dans la syringomyélie, *C. R. Acad. Sci.*, 197, 1933, 792-794. Cf. also Von Skramlik, *op. cit.*, 238, who reports several other references to Bourguignon which we were unable to verify.

[15] L. J. Hut, *op. cit.*

For this reason, every *O* was given an extended period of training before the actual chronaximetric determinations were undertaken.

Observers. The *O*s for the entire course of experimentation were Mr. John W. Macmillan(*Ma*), senior assistant in psychology, Mr. Oliver L. Lacey(*La*), Sage fellow in psychology, and Mr. Henry L. Sisk (*Si*), assistant in rural education, with a minor in psychology. *Ma* had had previous experience in observing in this type of experiment, but *La* and *Si* had not, although they were familiar with the general methods and results of experimentation upon the skin.

Cold: (a) Aroused by adequate stimuli. First of all, *O* was trained in the experience and report of cold. During this period he sat at a table with his right forearm held rigidly in position by means of a plaster cast that was made especially for him. The cast exposed the dorsal surface of the forearm, upon which area all of the experimental work was done. This area was shaved at the beginning of every experimental hour, and a cross-grid of 2-mm. squares was stamped upon it. The cold spots were then located by means of punctiform stimulation with a Dallenbach thermoesthesiometer through which water was circulated in order to control its temperature.[16] The water, taken directly from the University mains, varied from day to day in temperature between 12° and 19° C. *O* was required to evaluate the intensity of the sensations aroused on a five-point scale: 1 being very weak, and 5 very strong. The intensive gradations were recorded so that only the stronger spots would be selected for further experimentation. In practice, the latter experiments were limited to spots of intensity 3 or 4, with now and then one of 5. The room temperature ranged from 19° to 21° C. from day to day. In these preliminary experiments, usually two areas were mapped at every experimental period. Training in the reporting of cold was continued until the *O*s were thoroughly familiar with the quality and their intensive reports of it varied over the range of the 5-point scale.

(b) Aroused by inadequate stimuli. Since the current which was to be used in determining the chronaxy was an inadequate stimulus, the *O*s were next stimulated with three different forms of inadequate stimuli in order that they would be familiar with cold, no matter how aroused.

(1) Pressure. Pressure, the most easily applied of the inadequate stimuli, and the one least likely to arouse distracting concomitants, was tried first. A blunt gutta percha point, which aroused no sensations of temperature when brought to bear upon the skin, was pressed lightly on a cold spot, and moved to and fro with a sort of 'stirring' motion. Under these conditions, cold was usually reported. It was not, however, aroused by a stationary pressure; movement was required.

(2) Prick. The second inadequate stimulus used was the prick of a needle. The needle was prepared by sharpening under a binocular microscope until the point was long and very sharp. When the skin had been softened by rubbing with soap and a rough paper towel, the application of this needle aroused only pain, and no pressure. In stimulating the cold spots, the needle was probed slowly into the skin. Frequently a clear pain as well as cold was aroused. It was difficult always to strike a cold spot directly, so that several jabs of the needle often were necessary before cold was aroused. When aroused, it was clear and in no way obscured by the accompanying pain.

(3) Electrical. Finally, a faradic current was applied by means of an induction

[16] Dallenbach, Some new apparatus, this JOURNAL, 34, 1923, 92 ff.

coil. The active electrode was a moderately heavy piece of copper wire, bent double so as to give a rounded point about 1 mm. in diameter. The passive electrode consisted of a sheet of copper covered with a padding of gauze on one side which was simply moistened with tap water, since this contained enough dissolved material to serve as a fairly efficient electrolyte. It was merely held in the hand opposite to the arm being stimulated. When the current was adjusted to the point of being slightly painful, this type of stimulation proved very effective in arousing cold. It is interesting to note that the intensity of the spot reported by *O* during mapping bore no observable relation to the ease of its arousal by faradic stimulation.

When these inadequate stimulations were accomplished, the attempt was made to measure the chronaxy of cold. After positive results with cold were obtained, the experiments upon warmth were undertaken.

Warmth: (a) Aroused by adequate stimuli. In the experiments with warmth, the general plan of procedure was the same as that for cold except that warm instead of cold water was circulated through the aesthesiometer. The water was warmed, by being passed over an electric coil, to a temperature of approximately 45°C. Training progressed with the adequate stimulus for warmth until the *O*s were thoroughly familiar with the 5-point intensity scale.

(b) Inadequate stimuli. Then the same three inadequate stimuli—pressure, prick, and faradic current—were administered, and proved as effective for warmth as they had for cold. At first, the *O*s had some difficulty with these observations, due to the more diffuse and indefinite nature of the sensation of warmth, but after a few days' training, they found that the difficulty had vanished. Warmth finally proved easy to observe, even in the presence of the pain and pressure which were aroused by the faradic current.

After the training in warmth, the work again was shifted to the measurement of chronaxy, this time for warmth. Thus the plan of experimentation was training in cold, chronaxy of cold, training in warmth, chronaxy of warmth. The strength-duration curves for cold and warmth, and a few measures of the chronaxy of pressure and of pain, were taken at the very end of experimentation.

Main Experiments

(1) Chronaxy measurements: (a) Cold. The actual chronaximetric determinations were made by means of the thyratron-tube chronaximeter described by Kreezer, Coakley, and Dallenbach.[17] This instrument delivers rectangular pulses of current, whose durations are controlled by means of a decade condenser. The active electrode, attached to the cathode, was the same as that used in the study by Neff and Dallenbach,[18] and consisted of a ¼ in. glass tube with a platinum wire sealed through one side. The lead ended in a silver wire which lay in contact with a cotton wick about 1 mm. in diam. kept moistened with a moderately strong salt solution. The indifferent electrode was a piece of copper screening covered with cheese-cloth moistened with salt solution, which *O* held in the hand opposite to the arm being stimulated, or upon which he laid the opposite arm.

The casts mentioned in the preliminary experiments were discarded when we

[17] G. Kreezer, J. D. Coakley, and K. M. Dallenbach, A thyratron chronaximeter: its operation and calibration, this Journal, 46, 1934, 641-647.

[18] Neff and Dallenbach, *op. cit.*, 633 f.

turned to the measurements of chronaxy because of their distracting effects. They were invariably cold, and though they could have been warmed to the physiological temperature of the arm, their use was omitted as unnecessary since there was no necessity of maintaining the arm in the same position from day to day. We subsequently, therefore, simply had *O* lay his arm upon a few towels spread on the table.

The procedure for the stimulation of cold was as follows. First a region on the dorsal surface of each forearm was shaved and two grids were stamped on each arm. The cold spots were then mapped by the continuous method,[19] usually until two spots of intensity 4 or 5 were found lying next to each other. In case the entire area was mapped without this occurring, the densest region of spots was selected for experimentation. Since one arm was mapped, and then the other, a period of about 10 min. usually elapsed between the mapping and the application of the electrical stimulus. One spot was always selected for stimulation from each grid. Next the rheobase for each of the selected spots was determined, by the short-circuiting of the usual control key of the chronaximeter, and the manual operation of a knife switch which yielded a pulse of rather long duration (about 0.5 to 1 sec.). The apparent crudeness of this method is rendered justifiable by the reliability of the rheobases obtained. The rheobases were always determined by an ascending series of voltages, which progressed by 1 volt steps until a sensation of cold was reported. After the rheobases for one arm had been obtained, experimentation was shifted to the other arm, so that a period of recovery intervened between this stimulation and the determination of the chronaxies. The chronaxy was obtained by doubling the rheobase voltage, and then gradually increasing the duration until cold was reported. It should be noted that these observations were extremely difficult. *The most important point of technique concerns the water used to moisten the active electrode.* If this water be at room temperature, its coldness either masks the faint cold aroused by the electrical stimulus, or it serves partially to adapt the cold receptors and thus to inhibit their response. In any case, the observation of cold under such circumstances is very difficult, if not impossible. It is necessary to warm the water well above physiological zero, and to warm the electrode wick before every stimulation by dipping it in warm water. Since the wick cools quite rapidly, due to its small mass, the result is that it quickly approaches physiological zero upon being applied to the skin, and thus eliminates both the masking effect of cold and the distracting and masking effect of a warm stimulator.

(b) Warmth. The method used in obtaining the measurements from the warm spots was identical, in most respects, to that for cold. The shaving, stamping, and mapping proceeded as before, except that on two of the experimental days three areas on each arm were used. The electrode, even in the case of stimulation of the receptors for warmth, was slightly warmed, since an extremely cold electrode, it was found, masked the warmth.

(2) Strength-duration curves. For the determination of strength-duration curves for cold and warmth a condenser-discharge chronaximeter, similar to that used by Bourguignon,[20] and diagrammed in Kreezer and Bradway,[21] was constructed. The

[19] Dallenbach, The temperature spots and end organs, this JOURNAL, 39, 1927, 402-427.

[20] Bourguignon, *La chronaxie chez l'homme*, 1923, 1-417.

[21] G. Kreezer and K. P. Bradway, *op. cit.*

effective voltages were obtained by applying an ascending series of intensities at various durations. A curve was determined for each arm of every *O*.

(3) Chronaxy of pain and pressure. To serve as comparative measures, six determinations of the chronaxy of pain and of pressure were made upon every *O*. Three areas were used upon each forearm, and the procedure was as nearly as possible identical with that explained by Neff and Dallenbach.[22] Identity of rheobase and chronaxy sensations was insisted upon, and the observations proved very easy.

TABLE I

RHEOBASES AND CHRONAXIES FOR COLD

Ma		*La*		*Si*	
Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)
50	0.876	50	0.388	50	0.596
54	.898	50	.397	50	.610
56	.883	51	.437	50	.621
56	1.016	51	.403	50	.596
53	.988	55	.448	52	.609
58	.988	54	.437	51	.591
51	1.030	50	.440	50	.609
51	.988	54	.420	50	.609
51	.883	55	.420	52	.596
53	.977	51	.403	52	.609
53	.911	57	.440	51	.673
53	.988	53	.420	51	.621
56	.988	54	.430	50	.610
56	.898	56	.448	50	.596
52	.988	53	.440	51	.621
53	.988	53	.437	50	.596
Av. 53.5	0.956	52.9	0.426	50.6	0.610
σ 2.2	0.053	1.8	0.033	0.8	0.019

RESULTS

(1) Chronaxies of cold and warmth. Tables I and II show the rheobases and chronaxies obtained for every *O* for every experiment with cold and warmth. Sixteen measures of each quality were made with every *O*. The average rheobase for cold was 53.5 v. σ 2.2 for *Ma;* 52.9 v. σ 1.8 for *La;* and 50.6 v σ 0.8 for *Si*. The chronaxies were 0.956 ms. σ 0.053 for *Ma;* 0.426 ms. σ 0.033 for *La;* and 0.610 ms. σ 0.019 for *Si*. It will be observed that there is a high degree of consistency in the judgments of every *O*. The rheobases are comparable as among the *O*s, but the chronaxies are considerably different.

For warmth, *Ma* yielded an average rheobase of 49.4 v. σ 3.8 and an

[22] Neff and Dallenbach, *op. cit.*

average chronaxy of 0.091 ms. σ 0.023, while the values for *La* were 44.8 v. σ 5.9 and 0.684 ms. σ 0.043 and for *Si* were 34.1 v. σ 15.1 and 0.662 ms. σ 0.060. The variability of the rheobases for warmth are greater than for cold, and especially is this true for *Si* whose values for warmth ranged from 10 to 52 v. The chronaxies, however, show no greater variability than in the case of cold. It should be emphasized that the *Os* found no difficulty in distinguishing cold from warmth under the conditions of the experiment. It occasionally happened that during a determination of

TABLE II

RHEOBASES AND CHRONAXIES FOR WARMTH

Ma		*La*		*Si*	
Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)
51	0.888	50	0.650	46	0.618
49	.894	54	.618	46	.618
48	.894	36	.659	32	.604
50	.907	46	.683	34	.766
51	.924	48	.665	18	.702
52	.907	49	.683	52	.665
52	.888	49	.697	49	.650
48	.907	34	.711	43	.611
48	.907	36	.611	14	.802
50	.901	38	.691	50	.636
52	.879	48	.683	47	.665
51	.948	48	.691	40	.618
51	.888	51	.702	22	.675
52	.940	42	.808	10	.566
36	.876	43	.697	15	.697
50	.956	45	.702	28	.697
Av. 49.4	0.907	44.8	0.684	34.1	0.662
σ 3.8	0.023	5.9	0.043	15.1	0.060

the chronaxy of warmth, cold would be aroused, and also contrariwise, but no confusion resulted. To demonstrate the statistical differentiation of the two sets of measures, the critical ratios between the chronaxies found for cold and warmth were calculated. For *Ma* the C.R. is 3.4, for *La* 18.6, and for *Si* 3.0. These all indicate mathematical certainty that the two measures are genuinely different.

(2) The strength-duration curves. The strength-duration curves for cold and for warmth are given in Figs. 1 and 2, respectively. The cold curve for *Si* is incomplete in the middle range, since he found that the current density in this region aroused so much pain and pressure as to mask the cold. The other *Os* give curves appearing much as the normal strength-duration curves obtained from homogeneous tissues generally. The curves

for warmth are also normal in shape, but those of *La* and *Si* show time-relations not corresponding to the chronaximetric determinations.

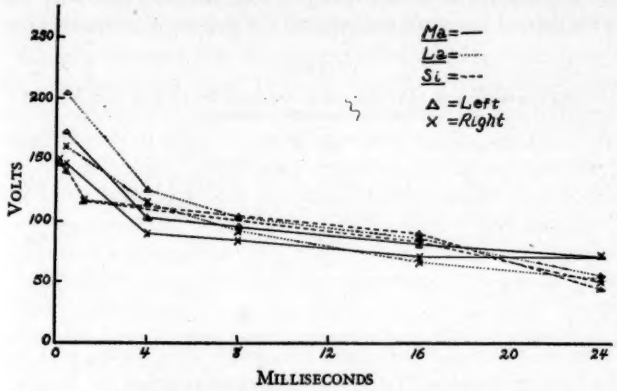

FIG. 1. STRENGTH-DURATION CURVES FOR COLD

(3) The chronaxies of the cutaneous senses compared. The fact that the chronaxies for all four of the cutaneous senses have been obtained from

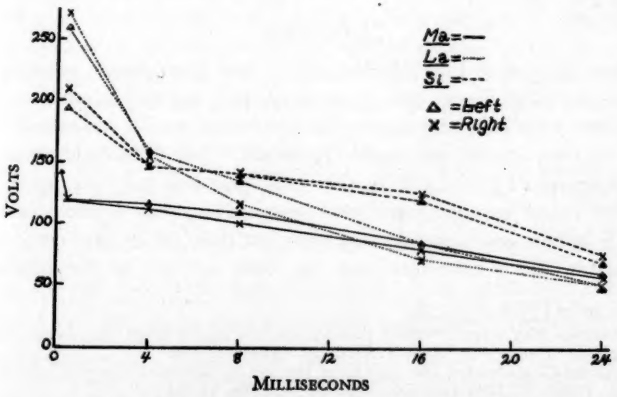

FIG. 2. STRENGTH-DURATION CURVES FOR WARMTH

the same *Os* makes possible a direct comparison of their values. Table III shows the average chronaxies, in milliseconds, for every *O* for pressure, pain cold, and warmth. The average pressure values check well with those

obtained by Neff and Dallenbach,[23] and are the shortest in the cases of *La* and *Si*. The pain chronaxies are next in magnitude for these two *O*s, and shortest for *Ma*. It should be noted that the values for pain quoted here were derived from one component, the shorter, of what appeared to

TABLE III

AVERAGE CHRONAXIES IN MILLISECONDS OF THE FOUR SKIN SENSES WITH RELATIVE RANK ORDERS IN PARENTHESES

O	Pressure	Pain	Cold	Warmth
Ma	0.548 (2)	0.255 (1)	0.956 (4)	0.907 (3)
La	0.257 (1)	0.572 (3)	0.426 (2)	0.648 (4)
Si	0.185 (1)	0.600 (2)	0.610 (3)	0.662 (4)

be a bimodal distribution of times. This point, it is hoped, will be the object of future research. The temperature chronaxies are the longest, but the relation between them is not consistent. Thus for *La* and *Si* warmth shows a greater magnitude, while for *Ma* it is cold which requires the greater time. The reversal of the relation between pain and pressure shown by *Ma* is paralleled by the reversal for cold and warmth.

DISCUSSION

Since the work of Blix,[24] Goldscheider,[25] and Donaldson,[26] which established the discreteness of pain, pressure, warmth, and cold as sense modalities, there have been several attempts to correlate specific end-organs with each of these sense departments. Donaldson,[27] and Goldscheider[28] themselves extirpated portions of the skin underlying cold and warm spots, but neither found any specialized end-organs. Häggqvist reported smooth muscle strands underlying the cold spots, but these results have never been verified.[29] When experimentation has been directed to the non-hairy

[23] Neff and Dallenbach, *ibid.*

[24] Magnus Blix, Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven, *Zsch. f. Biol.*, 20, 1884, 141-156.

[25] Alfred Goldscheider, Die specifische Energie der Temperaturnerven, *Monatshft. f. prac. Derm.*, 3, 1884 (see Ges. Abh., Bd. I, 1898, 53-76).

[26] H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 408 f.

[27] Donaldson, *ibid.*

[28] Alfred Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerven beim Menschen, *Arch. f. Anat. u. Physiol., Physiol. Abtheil., Suppl. Bd.*, 1886, 191-227.

[29] Gösta Häggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 44, 1913, 57 ff.

regions of the body, such as the cornea,[30] or the prepuce,[31] it has been possible to demonstrate a correlation between Krause end-bulbs and cold spots. Thus Strughold and Karbe believed that they had shown the coincidence of cold sensitive spots on the cornea and Krause end-bulbs. Bazett and McGlone reported that Krause end-bulbs underlay cold spots, and Ruffini cylinders warm spots. Unfortunately, the demonstration was not direct, but involved the so-called 'population' method, in which the number of sensitive spots in a given area is correlated with the number of end-organs of various kinds found upon sectioning that area. When the hairy regions of the body have been the object of attention, no such correlation has ever been reported. Dallenbach[32] and Pendleton[33] both have failed to find specialized ending of any sort, and Dallenbach suggested that perhaps some kind of free nerve endings were responsible for the reception of temperature changes.

There have been several non-experimentally derived suggestions as to the probable end-organs for the temperature senses, among which the earliest seems to be that of Von Frey.[34] It was he who first proposed Krause end-bulbs as the receptors for cold, and Ruffini cylinders for warm. This seems to be the accepted correlation among physiologists.[35] Kiesow suggested that the erectores pilae muscles might be involved in the sensing of cold.[36] Head made the interesting observation that more than one type of end-organ might be responsible for each of the two senses.[37] Bourguignon proposes that Meissner's corpuscles and the free nerve endings of the skin coöperate to give rise to temperature sensations.[38] His conclusions are based, partially, upon his inability to measure the chronaxy of cold, and

[30] H. Strughold and M. Karbe, Die Topographie des Kältesinnes auf Cornea und Conjunctiva, *Zsch. f. Biol.*, 83, 1925, 189-200. H. Strughold, Die Schwellen des Kältesinnes am Auge, *Zsch. f. Biol.*, 83, 1925, 201-206. H. Strughold and M. Karbe, Die Dichte der Kaltpunkte im Lidspaltenbereiche des Auges, *Zsch. f. Biol.*, 83, 1925, 207-212. H. Strughold and M. Karbe, Vitale Färbung des Auges und experimentelle Untersuchungen der gefärbten nervenelemente, *Zsch. f. Biol.*, 83, 1925, 297-308..

[31] H. C. Bazett, B. McGlone, R. G. Williams, and H. M. Lufkin, Studies in sensation. I. Depth, distribution, and probable identification in the prepuce of sensory end-organs concerned in sensations of temperature and touch; thermometric conductivity, *Arch. Neurol. & Psychiat.*, 27, 1932, 489-517.

[32] Dallenbach, *op. cit.*, this JOURNAL, 39, 1927, 425-427.

[33] C. R. Pendleton, The cold receptor, this JOURNAL, 40, 1928, 353 ff.

[34] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. kgl. sächs. Ges. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[35] S. W. Ranson, Cutaneous sensory fibres and sensory conduction, *Arch. Neurol. and Psychiat.*, 26, 1913, 1122 f.

[36] F. Kiesow, Zur Analyse der Temperaturempfindung, *Zsch. f. Psychol.*, 26, 1901, 231-246.

[37] Henry Head, *Studies in Neurology*, 1920, 63, 316.

[38] Bourguignon, Électrophysiologie, *C. R. Acad.*, 197, 1933, 792-794.

partially upon certain observed pathological changes in syringomyelia. One of the most interesting suggestions has come from Nafe.[39] He postulates that changes in temperature lead to contractions and relaxations of the blood vessels, notably the arterioles, of the skin, the stimulation afforded the nerve endings in the walls of these vessels by such changes in tone being perceived as cold or warmth. There are thus no specialized endings, and the patterns of contraction and relaxation are the determining factors. This hypothesis perhaps owes some debt to the suggestion of Goldscheider that the temperature sensitive spots follow the blood vessels.[40] Actually, then, nothing definite is known concerning the structure of the receptors for cold and warmth. The experimental results reported in this thesis, however, do shed a certain amount of light on this problem.

First of all, it would seem that the fact that reliably different chronaxies were obtained for cold and warmth would suggest specific, specialized end-organs subserving each modality. Bourguignon's suggestion concerning the coöperative action of Meissner's corpuscles and the free nerve endings must thus be discarded immediately. Also, Head's postulation of more than one type of ending for each sense department is disproved by the lack of a double chronaxy in either case, and by the absence of kinks or humps in the strength-duration curves. The possibility that the lack of correspondence between the strength-duration curves for warmth obtained from *La* and *Si* and the chronaxies might indicate the arousal of different receptors in the two cases may be countered by the following two considerations. First, the masking effect of the pain and pressure aroused by the current may have tended to raise the entire curve upward along the time parameter. Secondly, it may be, since the sensation arose quite late, according to the *Os*' reports, that the vaso-dilator fibers were being stimulated in this case, which would warm the tissues sufficiently to arouse the end-organs for warm. Against this latter suggestion is the possibility that the vaso-dilator fibers are iterative.[41] The question is still an open one, however, since the present curves appear similar to those obtained from visceral fibers.[42] It is further interesting to note that the values for the

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[39] J. P. Nafe, The pressure, pain, and temperature senses, *Handbook of General Experimental Psychology*, 1934, 1049 ff.

[40] Alfred Goldscheider, *op. cit.*

[41] Janina Hurynowicz, Über die normale und pathologische Chronaxie der vasomotorischen Nerven, *Dsch. Zsch. f. Nervenhlk.*, 129, 1933, 273-276.

[42] G. H. Bishop and Peter Heinbecker, Differentiation of axon types in visceral nerves by means of the potential record, *Amer. J. Physiol.*, 94, 1930, 170-200; A functional analysis of the cervical sympathetic nerve supply to the eye, *ibid.*, 100, 1932, 519-532.

chronaxy of warmth reported by Bourguignon, about 5 ms. to 7 ms., correspond roughly to the time values of the curves we have determined.[43] Possibly, then, both Bourguignon's results and the present strength-duration curves refer to vaso-dilator fibers.

The chronaximetric results further suggest grave difficulties for Nafe's proposals. In the first place, it is unlikely that the same type of shock would arouse in one case cold, and in another warmth, since it would seem that the smooth muscles of the blood vessels should respond consistently in the same way to the same stimulus. Furthermore, the times obtained seem too short for smooth muscles. The possibility that the nerve endings in the blood vessels are being stimulated directly is ruled out by Nafe himself, since he postulates no specific nerve endings, and emphasizes the importance of the patterning of the tonic changes. That the vaso-motor fibers were being stimulated directly is rendered unlikely by the relatively longer times to be expected for the chronaxy of such nerves.[44] The variability of the rheobase for warmth as contrasted to that for cold is a damaging fact. It would seem that this result, coupled with the very consistent chronaxies in the two cases, would suggest that the warm receptors vary much more in depth than do the receptors for cold. Since for Nafe the essential receptor mechanism is in both cases the same, this should prove very difficult of explanation in terms of his theory.

On the positive side, the chronaximetric results can make only a few suggestions. The primary conclusion, of course, is that cold and warmth are subserved each by a single, specialized receptor mechanism. This follows from the reliable difference between their chronaxies, and from the extreme reliability of the values found. The variability of the rheobase for warmth further suggests end-organs varying greatly in depth. It might be profitable to select spots showing a very low rheobase, and thus shallow receptors, for extirpation. Of course, the chronaxies do not make possible specific identification of the end-organs. The relatively high chronaxies for the temperature senses, as compared with pain and pressure, is, however, very interesting. This fact leads to the conclusion that relatively insensitive mechanisms are involved. It thus tends to lend support to Dallenbach's conclusions that relatively non-specialized free nerve endings of some sort are responsible for the temperature sensations. The very high rheobase might also suggest either end-organs lying deeper in the skin, or end-organs surrounded by heavy layers of tissue which act as insulators.

[43] Bourguignon, Chronaxies normales des terminaisons servitires cutanées chez l'homme, *Amer. J. Physiol.*, 90, 1929, 294.
[44] Bishop and Heinbecker, *op. cit.*

It may be concluded, then, that the results here reported show that there are specific, differentiated receptors for cold and for warmth. Although they can yield no definite suggestions as to the nature of these end-organs, they do place certain limitations upon hypothesizing in this field, as has been shown above.

THE ROLE OF MOTIVATION IN LEARNING WITHOUT AWARENESS

By JAMES GRIER MILLER, Harvard University

The present evidence on learning to discriminate subliminal sensory stimuli is scanty, and there is none on the effect of motivation in such learning. This is a report of an experiment showing that changes in motivation affect the acceleration of the curve of the acquisition of skill in discriminating between subliminal stimuli.

ANTECEDENTS

In his presidential address in 1916,[1] Watson took pains to forestall one line of criticism of his view of the conditioned reflex by showing that it is not necessarily or essentially a 'voluntary reaction.' At that time *voluntary* and *conscious* were to some psychologists strangely synonymous. To prove his point Watson cited preliminary experiments which he had performed. He had found that it is possible to some degree to condition pupillary contraction, and he insisted that, since this contraction is mediated solely by the autonomic nervous system, conditioning can not be an essentially 'voluntary reaction.' In contradiction Hamel,[2] from introspective reports, decided that the conditioned response is always dependent upon consciousness. His evidence for this assertion was that the latent time of the conditioned reflex approaches the reaction-time of conscious discrimination, which is much longer than the time for ordinary reflexes involving striated musculature. He made no attempts, however, to develop conditioning to imperceptible stimuli.

In 1922 Cason conclusively demonstrated pupillary conditioning,[3] and reported further that some *Ss* were not aware of the conditioning stimulus, and that none was aware of the pupillary changes. Later the same year Cason reported conditioning of winking.[4] He said that the *Ss* all insisted that they were not aware of the winks until after they had occurred, and that there were no accompanying introspective 'images' or 'affective elements,' at least until after the winks.

Cason and Katcher in 1933 said: "Although it has been demonstrated that subliminal stimuli can influence bodily activities, it has not yet been shown that a connection can be formed between a subliminal stimulus and a conscious or unconscious response by the process of learning."[5] They insisted that definite findings on this problem would have important theoretical and practical value. Therefore they

* Accepted for publication June 25, 1939.

[1] J. B. Watson, The place of the conditioned-reflex in psychology, *Psychol. Rev.*, 23, 1916, 89-116.

[2] I. A. Hamel, A study and analysis of the conditioned reflex, *Psychol. Monog.*, 27, 1919, (no. 118), 1-65.

[3] H. Cason, The conditioned pupillary reaction, *J. Exper. Psychol.*, 5, 1922, 108-146.

[4] Cason, The conditioned eyelid reaction, *ibid.*, 153-196.

[5] H. Cason and N. Katcher, An attempt to condition breathing and eyelid responses to a subliminal electric stimulus, *ibid.*, 16, 831-842.

attempted to condition breathing and eyelid responses to a subliminal electric stimulus. Although a variety of procedures was followed, they were unable to condition either response. They suggested better techniques which they thought might succeed. In the same year Hudgins by conditioning developed the pupillary reflex so that *S*'s pupil would contract if he thought the word 'contract.'[6] This was learning without awareness and beyond voluntary control, for two reasons: (1) because 10 of the *Ss* were naïve and did not know that their pupillary reactions were being conditioned; and (2) because there are no afferent neurones passing from the iris.

Hamel had planned,[7] but never performed, experiments to discover whether the introspective limen and that determined by the method of conditioned response were identical. In 1933 Newhall and Sears reported experiments which seemed to indicate that finger withdrawal can be conditioned to stimuli below the statistical psychophysical limen, but they were uncertain whether this conditioning is accompanied by consciousness.[8]

Corroborating some evidence of Thorndike in 1932,[9] Thorndike and Rock devised, and in 1934 published the results of two experiments which purported to show that learning may proceed without awareness of what is being learned or intent to learn it.[10] In one of these, a free-association experiment, the *Ss* were told that the task was to respond to the stimulus-words rapidly. Their answers were judged right or wrong by *E*, and the *Ss* were told this judgment was made on the basis of an arbitrary list of 'right' responses for each stimulus-word. Actually words with sequential connections were called right and rewarded, and *Ss* learned to give such free associations rather than other words, such as those connected with the meaning of the stimulus-word. Because the learning curves rose slowly, the *Es* assumed that the *Ss* were not aware of the task, on the grounds that with insight the curves would have risen immediately to 100% correctness. *Es* also devised a skill-test on the same principles which gave much the same results. Irwin, Kauffman, Prior, and Weaver did not believe that the gradualness of the rise of the learning curve was an adequate criterion of learning without awareness,[11] and criticised Thorndike and Rock on that point. Their criticism was based on experiments which they performed using Thorndike and Rock's association-test, in which the *Ss* were told the purpose of the experiment half way through it. This did not make the learning curves rise rapidly to 100%. In a reply, Thorndike and Rock admitted the invalidity of their criterion of gradualness, but said that their experiment still contained much evidence for learning without awareness.[12] They had not at any time asked the *Ss* whether or not they were aware of the task, and no other test of unawareness was proposed. Irwin closed

[6] C. V. Hudgins, Conditioning and the voluntary control of the pupillary light reflex, *J. Gen. Psychol.*, 8, 1933, 3-51.

[7] I. A. Hamel, *op. cit.*, 24-26.

[8] S. M. Newhall and R. R. Sears, Conditioning finger retraction to visual stimuli near the absolute threshold, *Comp. Psychol. Monog.*, 9, 1933, (no. 43), 1-25.

[9] E. L. Thorndike and others, *The Fundamentals of Learning*, 1932, 207 ff.

[10] E. L. Thorndike and R. T. Rock, Jr., Learning without awareness of what is being learned or intent to learn it, *J. Exper. Psychol.*, 17, 1934, 1-19.

[11] F. W. Irwin, K. Kauffman, G. Prior, and H. B. Weaver, On 'learning without awareness of what is being learned,' *ibid.*, 823-827.

[12] Thorndike and Rock, A further note on learning without awareness of what is being learned, *ibid.*, 18, 1935, 388-389.

the debate with a short note reasserting his earlier request for such a criterion.[13]

Silverman and Baker[14] tried unsuccessfully in 1935 to condition to a subliminal shock an increase in breathing or in foot pressure. In 3 out of 12 *Ss*, however, increased frequency of the eyelid response was conditioned, and 2 of these were reconditioned. In former experiments the stimulus had been known and the response unknown; here the stimulus was unknown and the response possibly known. Neet claims to have corroborated part of this work.[15]

More recently Baker[16] has succeeded in conditioning the pupillary response below what he refers to as "the absolute limen of awareness of the stimulus," which is lower than the "psychophysical threshold," which was used by Newhall and Sears. Baker has experimented in detail with the nature of subliminal conditioning.

Problems

In an earlier paper,[17] the author gave experimental evidence that it is possible to discriminate between subliminal geometrical figures without awareness either that a visual experiment is in progress or what the source of the subliminal stimuli is. The problems of this second study are:

(1) Can *Ss* learn to improve their subliminal discrimination even though they are unaware that they are taking part in an experiment in visual perception?

(2) Is subliminal learning like supraliminal learning in that the acceleration of the curve of acquisition is affected by changes in motivation?

(3) Is the hierarchy of motivations for subliminal learning like the hierarchy for supraliminal learning?

Apparatus and Procedures

Four learning experiments were conducted. In each succeeding one the motivation was changed in a way that was thought, on the basis of the literature on learning with awareness, would accelerate the function. In the last two experiments indications of learning were obtained.

The basic apparatus used in all these experiments was described in detail by the author in the earlier article.[18] *Ss* were seated in a dimly-lighted room facing an 8 x 10 in. transparent mirror set in the opposite wall. *S*'s chair was about three yards from the mirror. Without *S*'s knowledge geometric figures were projected

[13] Irwin, A rejoinder, *ibid.*, 389.

[14] A. Silverman and L. E. Baker, An attempt to condition various responses to subliminal electrical stimulation, *ibid.*, 246-254.

[15] C. C. Neet, A comparison of verbal, manual, and conditioned-response methods in the determination of auditory intensity thresholds, *ibid.*, 19, 1936, 401-416.

[16] L. E. Baker, The pupillary response conditioned to subliminal auditory stimuli, *Psychol. Monog.*, 50, 1938, (no. 223), 1-32.

[17] J. G. Miller, Discrimination without awareness, this JOURNAL, 52, 1939, 562-578.

[18] Miller, *op. cit.*, 566-567. The apparatus was purchased by Harvard University's Maria E. McMaster Fund for psychic research.

silently upon the back of the mirror at various intensities. The figures used were those on J. B. Rhine's ESP cards—*circle, plus, star, square, wave.* When projected on the mirror the figures were enlarged to almost the full size of the mirror.

E sat behind *S* in a small room connected with *S*'s room through an open door. He sat behind a screen facing a panel from which he could control remotely the exposure of the cards and the illumination at which the cards were projected. This illumination was regulated by a variac, which gave nothing at 0 volts, and a comparatively dim, though definitely supraliminal, image at its top reading, 135 volts. By a periscope *E* could tell what figure was being presented on the mirror, even at illuminations well below the limen.

All *S*s were naïve, unaware of the projection apparatus and the purpose of the experiment.

Experiment A: Learning motivated by desire to please E. When *S* was taken into the experimental room, he was immediately led up to inspect the mirror on the front wall. *E* took ample precaution to see that *S* had sufficient evidence that it was a real mirror, being certain that he saw his reflection. This fact, however, was not emphasized so strongly that *S* had any cause to question that it was a regular mirror. With no light behind it gave reflections like an ordinary mirror.

S was then seated in his chair facing the mirror, shown the ESP cards, and told about Rhine's work. Then he was given oral directions approximately as follows:

It is a common procedure in clairvoyance experiments for *S* to gaze into a crystal to help him find the answer he wants. This is usually done when *S* tries to picture three-dimensional objects. In this experiment, however, you are going to work with the figures on the flat surfaces of ESP cards. I am asking you, therefore, to stare into the flat surface of this mirror. All during the experiment I want you to stare into the mirror, and not to move your head or your eyes. You may, however, look at the mirror with any part of your eyes that you wish—either fovea or periphery. When your eyes tire, tell me, and we shall rest.

The purpose of this experiment is to see whether, with experience, you can learn to do better in calling the ESP cards correctly than you can at present. I shall go into the room behind you and sit at the table behind the screen. Sitting there I shall expose the cards one by one. When I expose a card, I shall ring a signal bell. Please try to picture one of the five figures on the mirror as approximately 6 x 6 in. in size, that is, about one inch from either side of the mirror and two from the top and bottom. Report the first figure that you picture on the mirror. If you cannot get any cue from the mirror to help you, guess one of the figures, but say that it is a guess. As soon as you have mentioned a figure, I shall tell you whether you were right or wrong, and, if you were wrong, I shall tell you what the right one was.

Please report also what kind of image you see on the mirror, if you see one; that is, whether it is a memory image, a thought image, an imaginatory image, an hallucinatory image, an after-image, or an actual physical image.

Please use the following names for the cards: *plus, wave, star, circle, square.*

Do not figure that out of every 25 calls you must call 5 of each figure. Though an equal number of each figure will be used, the cards are in a large pack, and it is quite conceivable that chance may turn up a long run of any figure.

As a matter of fact 200 cards were used, and each time before the pack was exhausted, the cards were reshuffled and put back into the machine, so that the

pack was to all intents of indefinite size. Since *S* did not know that the cards were being projected from behind the mirror, it was natural that he should assume from the directions that *E* was exposing them behind him. This was the impression of all *Ss* until the apparatus was shown them at the end of the experiment.

In Experiment A a total of 12 series of 25 cards to call, three series a day every other day, were given to *F, G, H* and *I*. In the case of *F*, who was slow in calling the cards, only two series could be given the first day, but over the four-day period he was given 12 series.

To be sure that the *Ss* were never aware of the stimuli four precautions were taken: (1) The cards were projected at a constant illumination represented by 80 volts on the variac (about .0173 footcandles). For naïve *Ss*, like all in these experiments, it was found in earlier work with this apparatus[19] that none was ever aware of stimulus-objects presented at an illumination below 90 volts. The point of onset of the function of their awareness was about 95 volts. Sophisticated *Ss* in the earlier work, who knew all about the apparatus, however, were aware of figures at lower intensities. The point of onset of the function of their awareness was about 70 volts. Although no sophisticated *Ss* were used in the present work, the illumination used in Experiments C and D was 70 volts, in order to make as stringent a test as possible. Moreover, the data for Experiments A and B were prorated to the values they would have had if the experiments had been done at 70 volts (.0163 footcandles). This prorating was based on functions for discrimination at various voltages determined in the earlier experiment.[20] (2) A second precaution to make sure that the *Ss* were never aware of the stimuli was to make them report what kind of images they saw after each series of calls. In no case was an actual physical image reported. (3) A third precaution was to record the point at which *S* first saw an actual physical image at the end of the experiment, by raising the illumination until he reported one. This never occurred below 120 volts. Moreover, *S*'s limen was taken afterward by the method of limits. It was never below 90 volts. (4) A final precaution was to ask *S*, after showing him the apparatus, if he had suspected that images were being projected on the mirror. *Ss* always replied in the negative and exhibited surprise at the set-up of the experiment.

Because in Experiments C and D it was thought advisable to use discrimination between only two of the geometrical figures instead of all five, the results for all experiments will be given not in percentage of correct calls, but in terms of an arbitrary scale of degree of discrimination which will make them directly comparable. This scale was developed as follows: When five cards were used, 20% of the calls would be correct by chance. Discrimination would then be indicated by series of calls averaging consistently above 20%. Therefore the interval between the chance percentage and 100% (an interval of 80%), which is indicative of discrimination, was divided into 100 equal units of degree of discrimination. When only two figures were used, 50% of the calls would have been correct by chance, and results consistently between 50% and 100% would have been indicative of discrimination. In Experiments C and D using two figures, then, the interval between 50% and 100% was in like manner divided into 100 equal units of degree of discrimination, which were then comparable with the units from the experiments using five figures.

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[19] Miller, *op. cit.*, 577.
[20] Miller, *op. cit.*, 572.

In Experiment A the percentage of correct calls for a *S* for one day was determined, and —20% algebraically added to it. If the percentage of correct calls had been above 20, this would give a positive number; at 20 it would give 0; and below 20, a negative number. This was then multiplied by the factor 10/8 to convert it to the arbitrary scale. (If two figures only had been used, the conversion procedure would have been the same, except that the factor would have been 2 instead of 10/8.)

The results for Experiment A, prorated for 70 volts and reduced to the arbitrary scale of degrees of discrimination are shown in Table I.

Since the results from Experiment A gave no evidence of learning (all the data being near the chance value), an attempt was made to increase the motivation. That increased motivation was necessary was indicated by

TABLE I

RESULTS OF EXPERIMENT A

S	Number of practice sessions			
	1	2	3	4
F	3.1	9.7	−0.3	1.5
G	−0.3	−0.3	4.9	−1.9
H	−0.3	1.5	−1.9	−6.9
I	−0.3	1.5	3.1	11.5
Av.	0.6	3.1	1.6	1.0

the fact that all the *S*s reported discouragement and frustration because they did not share *E*'s apparent belief in 'mind reading' and thought the task impossible, and because they were bored with the monotony of calling hundreds of cards. Counteracting this reaction of the *S*s was a continual problem in these experiments.

Experiment B: Learning motivated by apparent increasing success. J, K, and *L,* were used as *S*s. The task was made somewhat easier by raising the illumination voltage to 90 volts, only five volts below the point of onset of the function of awareness for naïve *S*s, and by interspersing figures at a higher voltage (110 volts). These cards at 110 volts were not counted in the scores, however, but were introduced merely for encouragement. In the first series of 25, all the cards were exposed at 90 volts; in the next series, two at 110 volts were interspersed, one near the beginning of the series, and one near the end; in the next series, four at 110 were interspersed; in the next, six; in the next, eight; in the next, 10; and in the next, 12. The same number of series was given each of four sessions, every other day. The cards at 110 volts were called correctly more often, but 110 volts was below the limen of awareness for naïve *S*s. The

effect of interspersing these cards, therefore, was to make the *Ss* think they were doing better with each succeeding trial. Otherwise the procedure was the same as in Experiment A.

The motivation was quite effective for *J*, and slightly for *L*. *K* developed a theory during the third session that the purpose of the experiment was to give cues as to which card was being exposed by the length of time of ringing the signal bell, and so took his cues from the bell rather than the mirror. His results were therefore poor for the last two sessions. Since he did not report his theory until the end of his fourth session, his data are included for the sake of completeness.

TABLE II

RESULTS OF EXPERIMENT B

S	Number of practice sessions			
	1	2	3	4
J	2.5	2.5	7.5	30.0
K	−5.0	5.0	−10.0	−10.0
L	2.5	7.5	10.0	2.5
Av.	0.0	5.0	2.5	7.5

In Table II are the results for Experiment B, prorated for 70 volts, on the arbitrary scale of degrees of discrimination. Since it appeared from the results of this experiment that even if learning did occur under its degree of motivation, it would not be very rapid, another attempt to increase motivation was made.

TABLE III

RESULTS OF EXPERIMENT C

S	Number of practice sessions			
	1	2	3	4
M	0.0	6.0	24.0	20.0
N	0.0	10.0	38.0	16.0
P	−10.0	2.0	4.0	14.0
Q	2.0	4.0	8.0	12.0
Av.	− 2.0	5.5	16.0	15.5

Experiment C: Learning with monetary reward and punishment. Four naïve *Ss*, *M*, *N*, *P*, and *Q*, were given four series of 25 cards each day for four consecutive days. They were bet five cents on every card: they received five cents for every card right, and paid five cents for every one wrong. Because it would simplify the betting, and because it would make the discriminatory task somewhat easier, only two of the five figures—*circle*

and *plus*—were used in Experiment C. In order to be doubly certain that an adequate test was made, and that *S*s were unaware of all the cards, the illumination was lowered to 70 volts. Otherwise the procedure was as in Experiment A.

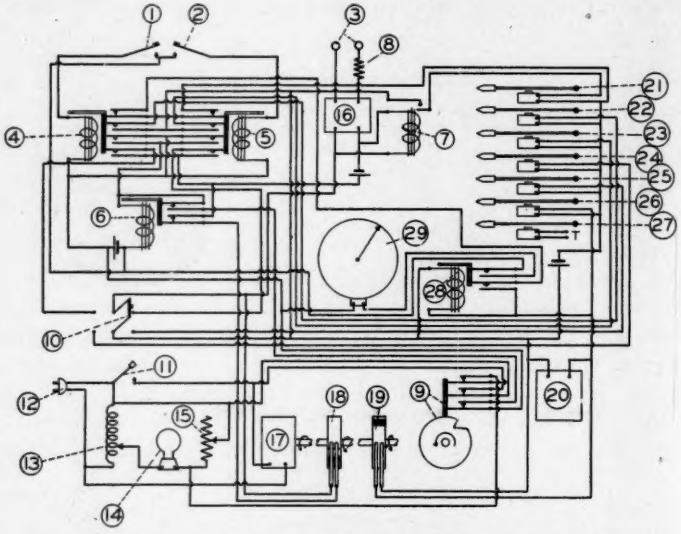

FIG. 1. WIRING DIAGRAM OF APPARATUS FOR EXPERIMENT D

1, *S*'s left key (*plus*); 2, *S*'s right key (*circle*); 3, shock electrodes; 4, 5, 6, 7, 28, relays; 8, 10,000 ohm resistor to counteract changes in *S*'s resistance; 9, motor driven 3-pole, single-throw switch; 10, double-pole, double-throw switch which *E* uses to close circuit for *circle* or *plus*, according to which is projected; 11, single-pole, single-throw main switch which closes circuit through motor of 9; 12, to 110-volt power supply; 13, variac; 14, lights furnishing illumination to project image; 15, variable dimmer resistor to control illumination before 9 shunts it out, so *E* can see card through periscope before it is exposed to *S*; 16, inductorium; 17, synchronous motor driving 9, 18, 19; 18, 19, commutator switches; 20, signal bell; 21, shock kymograph signal magnet (k.s.m.); 22, left key k.s.m.; 23, right key k.s.m.; 24, k.s.m. indicating 10 set for left key (*plus*); 25, k.s.m. indicating 10 set for right key (*circle*); 26, k.s.m. indicating illumination on; 27, k.s.m. to second-interval timer; and 29, chronoscope. Mr. Ralph Gerbrands is responsible for much of this electrical work.

The results of this experiment, which was carried on at 70 volts, on the arbitrary scale of degrees of discrimination, appear in Table III.

Finally an even stronger motivation was tried.

Experiment D: Learning with punishment by electric shock and reward by praise. Three naïve *S*s, *P*, *T*, and *V*, were used in this experiment.

An apparatus, diagrammed in Fig. 1, was used. *S* pressed the left key if he thought the card to be a *plus*, and the right if he thought it to be a *circle*. He had electrodes on his wrist, and, if he pressed the wrong key, he automatically received a shock immediately. If he pressed the correct key, he got no shock. If he pressed no key in three seconds, he got a shock. He was praised whenever he made a right choice. An automatic

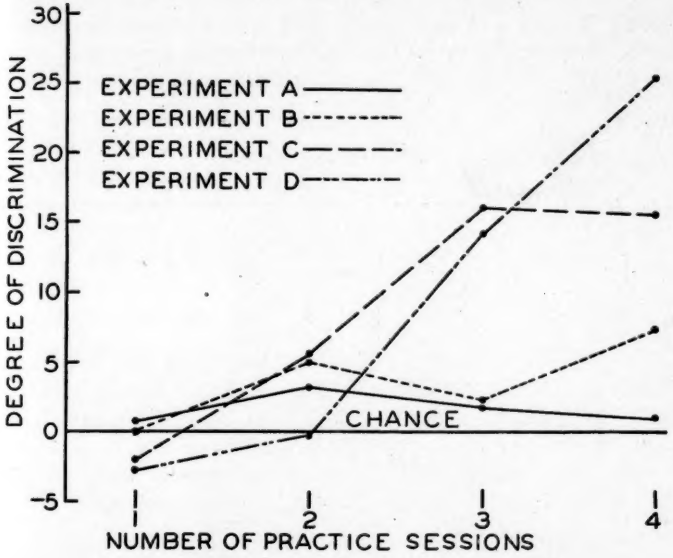

FIG. 2. AVERAGE RESULTS FOR FOUR EXPERIMENTS
Degree of discrimination is measured in arbitrary units explained in the text.

kymograph record was made, and the time from the flashing on of the lights projecting the card and the simultaneous ringing of the signal bell to the pressing of the key was recorded. This last observation was made in order to discover which was more rapid: the correct or the incorrect choice, the left or the right hand. No significant differences were found in either case.

Five series of 25 were run for each *S* for four sessions, every other day. The illumination was at 70 volts. The results, on the arbitrary scale of degrees of discrimination, are shown in Table IV.

Discussion

Fig. 2 summarizes graphically the average results of the four experiments. This figure definitely indicates that learning to discriminate subliminal cues is possible when *Ss* are unaware that they are taking part in an experiment in visual perception. It indicates that motivational factors determine the acceleration of the curve of acquisition, as is shown in the differences between the curves of Experiments A and B and those of C and D. It is well known in learning with awareness that monetary reward and punishment, electric shock and praise are stronger motivations than desire to please *E*, or such a desire combined with increasing

TABLE IV

Results of Experiment D

S	Number of practice sessions			
	1	2	3	4
P	−8.8	−8.8	12.0	45.6
T	−2.4	12.0	23.2	16.8
V	2.4	−4.0	21.6	13.6
Av.	−2.9	−0.3	14.2	25.3

success. This difference has also been shown to be true of subliminal learning, which to this extent is like supraliminal learning. Baker has said that conditioning above and below the limen are different in that it is easier to condition to a subliminal stimulus, and that the conditioning appears more suddenly.[21] His work indicates ways in which subliminal and supraliminal learning differ, while the present study shows one way in which they are similar.

It is important to notice that in all the present experiments *S* was aware of the motivation even though he was not aware of the stimulus. This differentiation between awareness of stimulus or of response, on the one hand, and awareness of motivation, on the other, is important, and it has not often been carefully made by clinicians. Such techniques as have been employed here might well be used to determine which specific motivations are of most effect in unaware activities. Freudian theory on this point could thus be subjected to laboratory experimentation. Masochists might learn better to the tune of electric shocks than to praise. Individual differences might be determined by such techniques. The variations between *Ss* in the present results indicate that such deviations are great.

[21] L. E. Baker, The pupillary response conditioned to subliminal auditory stimuli, *Psychol. Monog.*, 50, 1938, (no. 223), 16.

Three experimental criteria of lack of awareness in learning have been used in the literature: (1) gradualness of rise of the learning curve;[22] (2) asking *S* if he is aware of the stimulus;[23] and (3) maintaining the stimuli below some psychophysical limen. That the first criterion is inadequate Irwin, Kauffman, Prior, and Weaver have proved.[24] The second is adequate, but entails the difficulties and dangers of introspective report. The third is the most satisfactory. There has been some disagreement as to the best method of determining the psychophysical limen. Baker used what he called "the absolute limen of awareness of the stimulus,"[25] which he considers a better measure than the "psychophysical threshold," such as Newhall and Sears used. This use of the word *limen* is unjustified, because it does not fit the accepted usage. To psychologists *limen* has traditionally meant a statistically determined point, usually the point where the sensation is reported 50% of the time. Baker, however, means by *limen* the lowest point at which it is ever possible to have awareness. This is the lowest point of the psychophysical function of awareness, for which the author has suggested the term "the onset point" of the function of awareness.[26]

Aside from his terminology, however, Baker is right. If *unaware* is defined as *below the psychophysical limen,* difficulties will arise. At an intensity just below the limen *S* will be able to report almost half of the stimuli. Learning to react to these stimuli would not be learning without awareness in the ordinary sense. Therefore all the data in our experiment have been prorated for 70 volts, below the lowest possible point of onset of awareness.

SUMMARY

This experiment has shown: (1) that learning to discriminate geometric figures at subliminal intensity can occur without *S*'s awareness that he is taking part in an experiment on visual perception; (2) that the acceleration of such learning curves is affected by changes in motivation; and (3) that the relative strengths of certain motivations for learning without awareness are in general comparable to those for learning with awareness.

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[22] E. L. Thorndike and R. T. Rock, Jr., Learning without awareness of what is being learned or intent to learn it, *J. Exper. Psychol.*, 17, 1934, 1-19.

[23] *E.g.* H. Cason, The conditioned eyelid reaction, *ibid.*, 5, 1922, 153-196.

[24] F. W. Irwin, K. Kauffman, G. Prior, and H. B. Weaver, On 'learning without awareness of what is being learned,' *ibid.*, 17, 1934, 823-827.

[25] Baker, *op. cit.*, 2.

[26] J. G. Miller, Discrimination without awareness, this JOURNAL, 52, 1939, 574-577.

THE PITCH OF COMPLEX TONES

By LLOYD A. JEFFRESS, University of Texas

In a previous article the writer suggested that the stability of pitch of a complex tone in the absence of its fundamental might be similar in nature to the perceptual completion of fragmentary visual patterns.[1] The present paper describes a series of experiments undertaken to test this hypothesis.

Fletcher reports a number of experiments in which a complex tone with the fundamental missing, and in some cases with the fundamental and several overtones missing, is compared in pitch with "a standard, giving a fairly pure tone."[2] He employed a variety of complex tones from organ pipes, sung vowels, violin, piano and clarinet. The Bell Telephone Laboratories have issued a phonograph record (B.T.L.-5) illustrating the phenomenon. Fletcher also reports a series of similar experiments in which the source of the complex tone is a vacuum-tube generator capable of producing the 10 frequencies 100~, 200~, . . . 1000~. In most of the experiments his *Ss* reported that the incomplete complex tone had the pitch of the missing fundamental. When the tone generator was used, the pitch of the composite was the pitch of the fundamental even when only three frequencies, *e.g.* 700~, 800~, 900~, were sounded at an intensity level of about 90 db. Later, Fletcher repeated some of these experiments using a loudspeaker as the source of sound instead of the telephone receiver employed in the first series.[3] The results were similar. In almost every case in both series when the sound level was sufficiently high (usually above 45 db.), the pitch reported by the *Ss* corresponded to that of the "fundamental," *i.e.* of the common difference between the frequencies of adjacent partials. The exceptions occurred only in those cases where the common difference was not an integral quotient of the frequencies. Here, the *Ss* reported that the sound had no definite pitch, was like a noise, or was lacking in musical quality.

Fletcher explains the stability of pitch of a complex tone in terms of difference tones between adjacent partials. If two different frequencies of vibration are imposed upon a body so constituted that its displacement is not proportional to the applied force, the vibration of the body may be expressed as the sum of various simple modes having different frequencies. These frequencies will include such values as the sum of the two generator frequencies, the difference, twice the first, twice the second, etc., and the number and intensity of these extra modes of vibration will

* Accepted for publication July 15, 1939.

[1] L. A. Jeffress, The case for, and some implications of the place theory of hearing, *Psychol. Rev.*, 46, 1939, 31-45.

[2] Harvey Fletcher, The physical criterion for determining the pitch of a musical tone, *Phys. Rev.*, 23, 1924, 427-437; *Speech and Hearing*, 1929, pt. 4, ch. 2.

[3] Fletcher, Loudness, pitch, and the timbre of musical tones and their relation to the intensity, the frequency and the overtone structure, *J. Acous. Soc. Amer.*, 6, 1934, 59-69.

depend upon the nature of the non-linearity of the body's response. Of these, the frequency resulting from the difference between the generator frequencies is the one that concerns us here. If we consider the frequencies of the partials of a complex tone, we see that the difference in frequency between any two adjacent partials is the same as the frequency of the fundamental. This means that, if a complex tone with its fundamental missing is applied to a non-linear response system, we may expect the fundamental frequency to be supplied as a difference-tone by the response system. The ear is, of course, such a non-linear system and will generate difference-tones, as many experiments have shown. Fletcher estimates the intensity of the 100~ tone produced when the six frequencies 500~, 600~, 700~, . . . 1000~ are applied to the ear, and he concludes that its amplitude may amount to as much as 75% of the amplitude of the loudest partial, when the pressure amplitudes in the outer ear canal are about 100 dynes per sq. cm.[4]

In most elastic systems such as the ear, the departure from linearity of response is a function of the amplitude of the displacing force. When the amplitude is small, the system may be essentially linear and generate no difference tone. In accordance with this fact, Fletcher found that when the intensity of the three tones, 700~, 800~, and 900~, was reduced below 45 db., the 100~ difference-tone disappeared and the three tones were heard as separate tones.

If the phenomenon of pitch stability in the absence of the fundamental could be demonstrated at intensity-levels low enough so that the ear-generated difference-tones are below the threshold of hearing, Fletcher's explanation would be inadequate, and the analogy with vision mentioned above would be somewhat strengthened.

Experiments

(1) Preliminary. A preliminary experiment seemed to show that this might be the case. The writer and several other *Ss* listened to the record demonstrating pitch recognition (B.T.L.5-B) played at a low intensity level and agreed that the pitch did not change when the fundamental, and the fundamental and second partial were eliminated from the tone of the organ pipe. They did not agree in the case of the clarinet tone. An analysis of the organ pipe tone is shown in Table I. It indicates that the fundamental was by no means eliminated from the tone as recorded, and of course, explains why the subjects agreed about the pitch of the tone. In the case of the clarinet tone, the fundamental was effectively eliminated.

(a) Apparatus. The apparatus used in the subsequent experiments was the same throughout. Various organ pipes were used as the source of sound. A ribbon microphone was placed near the opening of the swell chambers of an organ in a moderate-sized auditorium and connected to a preamplifier in another room so distant that the direct sound of the pipes could not be heard. Between the preamplifier and the main amplifier was a switch that connected either a frequency-bridge, for eliminating any desired frequency, or an attenuator. The output of the main amplifier was connected either to the telephone receivers used in some of the experiments, or to a power amplifier and loudspeaker. A wave-analyzer

[4] Fletcher, *Phys. Rev.*, 23, 1924, 436.

connected across the amplifier provided the means for measuring the components of the sound. The telephone receivers were of the crystal type, and previous experiments had shown them to have a satisfactory frequency response over the range employed in the present experiments. The loudspeaker had a response which was substantially flat over a still larger range of frequencies. Neither produced appreciable distortion at intensities beyond any used in the experiments, and the amplifiers were free from distortion to an even greater degree.[8]

A switch in the output circuit permitted *E* to connect the loudspeaker or the telephone receivers either to the output of the amplifier, or to an oscillator capable of giving a very pure tone, or to both at once. Thus it was possible to present to the *Ss* the complex tone of an organ pipe, the complex tone with any component eliminated or partially eliminated depending upon the adjustment of the frequency-bridge, the pure tone of the oscillator, or the complex tone with a pure tone of any desired frequency added to it.

(2) Tests for the presence of a 'subjective tone.' To be sure that the difference-tone generated in the ear when the applied sound is of low intensity is really below the threshold of hearing, some method of estimating its intensity is necessary. *S*'s mere inability to discover its presence in the complex of sound is not enough. The following method proved to be very sensitive.

A complex tone with the fundamental eliminated was applied to the ears by means of the telephone receivers. Its constitution was approximately as follows: fundamental, 388~, less than 8 db.; 2nd partial, 56 db.; 3rd partial, 62 db.; 4th partial, 60 db.; 5th partial, 43 db.; 6th partial, 46 db.; 7th partial, 46 db.; 8th partial, 42 db.; other partials, negligible. While this complex tone was sounding, a pure tone from the oscillator was introduced. Its frequency was about that of the fundamental of the complex tone, and its intensity was adjusted until it was barely audible. The pitch was then varied slightly up and down several times a second and the intensity still further reduced until this warble was just audible. Its intensity proved to be about 45 db. This is, of course, somewhat below the value that would have been threshold for a steady tone, since the warble is more easily noticed against the background of steady tone from the organ pipe. The value, 45 db., is a rough practical threshold for a tone of the frequency of the fundamental under the conditions obtaining, *i.e.* in the presence of a complex tone, and in a room in which the noise level was between 30 and 40 db.

The oscillator was then adjusted to a frequency a few cycles per second higher than that of the fundamental and to an intensity that permitted it to be steadily heard (about 48 db.). Then the frequency-bridge was unbalanced to permit more of the fundamental to come through. When the intensity level of the fundamental was sufficiently high, the fundamental and the oscillator tone could be heard beating. A measurement of this intensity of the fundamental showed it to be about 28 db.

[8] The writer is grateful to Professor C. P. Boner of the Physics Department of the University of Texas for permission to use the apparatus, and for much helpful advice.

It is possible, by this technique, to detect the presence in the ear of a tone that is some 17 db. below the effective threshold. When the fundamental was eliminated from the complex tone applied to the ear, and the oscillator tone added, no beats could be heard, but if, by opening the doors to the auditorium, some of the direct sound of the pipe was admitted to the ears, the beats again became audible. This experiment seems to indicate that the method is sufficiently sensitive to detect in the ear the presence of a fundamental frequency, of whatever origin, strong enough to be of any value in determining pitch.

(3) Further preliminary experiments. It was the purpose of *E* at this point to present to the *Ss* complex tones devoid of the fundamental and sufficiently low in intensity so that the ear-generated fundamental was not detectible; then, if the pitch seemed to be that of the fundamental, to remove enough high partials from the remaining tone so that the pitch no longer seemed the same. This procedure was expected to give an indication of the way in which the pitch of the tone is dependent upon the spatial pattern the tone creates upon the basilar membrane.

These experiments showed that the pitch of such tones does not remain constant when the fundamental is removed. *Ss*, asked to decide whether the fundamental frequency or its octave best represented the pitch of the complex tone, sometimes chose one and sometimes the other, but usually reported that neither seemed a good match. One *S* with 'absolute pitch,' when asked what note he was hearing, *g* of 388~, reported *"g* and *d,"* which he identified as having the frequencies of the second and third partials.

These results are, of course in agreement with the work of Fletcher discussed above, but they suggest that a test of Fletcher's theory would be to raise the intensity-level of the tone until the pitch would remain that of the fundamental, and then to test for the presence of an audible ear-generated fundamental. *E*, however, was unable to find any such intensity-level. Even with sounds having an intensity-level of 100 db., the *Ss* reported that the pitch of the tone devoid of its fundamental seemed to be higher than the pitch of the complete tone, and higher than the pitch of the oscillator when it was adjusted to have the frequency of the fundamental. In many cases the octave of the fundamental seemed a better match.

These results led to the performance of a final experiment to see to what extent a larger group of *Ss* of varied musical experience would agree with the small number of musically trained *Ss* employed in these preliminary experiments.

(4) Final experiments. Thirty-six *Ss*, mostly young adults, were used in this experiment. Their musical training varied from about eighteen years for one *S* to none at all for the majority. Only one had had any experience with the organ.

(a) Procedure. The *Ss* were seated toward the front of a moderate-sized lecture-room, near the loudspeaker that was the source of sound. The complex tone

of an organ pipe (either with or without its fundamental) was presented to them alternately with the sound of the oscillator. If the pitch of the oscillator was adjusted to be that of the fundamental of the pipe, they were told that they were listening to combination "A"; if it was adjusted to be that of the octave of the fundamental, they were told that they were listening to combination "B." In each 'trial' the two 'combinations' were presented twice, and each 'pair,' complex tone and oscillator tone, eight times, *i.e.* two 'sets' of four.

Thus 'trial' 1 was as follows: "Combination A:" complex tone (open diapason, *g* 388~, fundamental present)—oscillator, 388~—complex tone—oscillator—complex tone—oscillator—complex tone—oscillator; "Combination B:" complex tone (same as before)—oscillator, 776~—complex tone—oscillator—complex tone—oscillator—complex tone—oscillator; "Combination A" again; "Combination B" again. At the end of the 'trial,' which required about 10 sec. to present, the *Ss* were asked to write "A" or "B" in the first blank on the sheets they had been given, according as the pitch resemblance in "A" or "B" seemed the closer. This was explained to them fully and several trials were given before the series was presented. No suggestion was made that either response was 'correct.' They were told that in the event their judgment was "very uncertain," they should place a question mark beside the response.

'Trial' 1 was followed by a second 'trial' with the same pipe, this time with the fundamental eliminated; and every 'pair' of 'trials' included one 'trial' in which the fundamental was present and one in which it was absent, the order depending upon chance. Ten different organ pipes were used, all of the same frequency, *g* 388~. After the ten pipes had been presented once, they were presented a second time in a different order. This made a series of forty 'trials' in all.

The sound level to be employed was measured in several parts of the room and for each tone was adjusted to be at least 70 db. The oscillator tone was made of about equal loudness, so that it was slightly higher in intensity level. After the series of presentations had been completed, an analysis of each of the twenty tones was made. The analyses are given in Table I and include the intensities of any partials that could conceivably contribute to the formation of a difference tone, *i.e.* of any having intensities as much as 10% of that of the strongest partial.

(b) Results. The *Ss*' reports were arranged in order upon the basis of the number of times individual *Ss* had matched the pitch of the complete complex tone with the pitch of the octave of its fundamental. *Ss* who did this for more than half the tones were discarded, as well as a few *Ss* who failed to follow the instructions. This selection left a group of twenty-six whose results were used.

Table II gives the results for this group. The first two columns are the percentage of *Ss* indicating that the complete tone had the pitch of the octave, the first column being for the first presentation of the pipe and the second column for the second presentation. The last two columns are the percentages of *Ss* indicating that the tone with its fundamental

missing had the pitch of the octave, the third column being for the first presentation of the pipe and the fourth column for the second presentation.

An inspection of Table II reveals that the *Ss* show a great deal of uncertainty about pitch. Even when asked to decide about the pitch of the complete tone, they often prefer the octave to the fundamental as

TABLE I

ANALYSIS OF TONES EXPRESSED AS PERCENTAGE OF GREATEST AMPLITUDE

Tone	Fundamental	Partials									
		1	2	3	4	5	6	7	8	9	10
B.T.L.-5B record organ pipe tone	present	22	10	32	53	100	53	13	*		
	absent	11	11	15	20	100	93	20			
	absent†	15	10	12	17	100	100	58			
Open diapason	present	37	100	12	4						
	absent	0§	100	22	11						
Violin	present	31	28	100	56	78	16	31			
	absent	0	12	83	75	100	20	66			
Tibia	present	100	30	7	3	7	2				
	absent	0	38	88	100	15	12				
Melodia	present	100	90	50	29						
	absent	0	100	62	62						
Kinura	present	73	25	45	36	91	100	50	23	27	9
	absent	0	8	23	26	85	100	47	26	32	11
Reed oboe	present	50	38	100	38	46	5				
	absent	0	22	100	47	67	13				
Flute	present	100	60	4	2						
	absent	0	100	13	11						
Open diapason phonon	present	100	64	7	15	2					
	absent	0	100	21	64	16					
Trumpet	present	100	35	16	2	9	11	3			
	absent	0	100	67	44	67	89	27			
Salicional	present	84	67	89	100	27	22	16	11		
	absent	0	27	68	100	36	34	18	16		

* Other partials less than 10%.
† and also absent.
§ Less than 0.1%.

a pitch-match. This seems to be especially true of those pipes in which the fundamental is relatively weak and which have a strong series of upper partials, such as the violin and the kinura. In the case of the tibia, the melodia, the flute, and the trumpet, which have a strong fundamental and a short series of higher partials, the *Ss* preferred the pitch of the fundamental. There is, however, a great deal of uncertainty and inconsistency in the responses. For example, the open diapason phonon has a short series of partials and a strong fundamental, but in the first series its octave was preferred by 42% of the *Ss*. It will be seen that when

the fundamental is eliminated, a majority of the *Ss* prefer the octave, except in the case of the tibia.

It seemed possible that including in the group so many *Ss* who preferred the octave to the fundamental as a pitch-match for the complete tone might obscure any tendency that existed among the more musically sophisticated *Ss* to prefer the fundamental frequency to the octave for the pitch of the tone with its fundamental eliminated. To investigate this possibility *E* selected the 10 *Ss* who voted the fewest times (not more

TABLE II

PERCENTAGE OF 26 *Ss* SELECTING THE OCTAVE OF THE FUNDAMENTAL

(Responses marked with "?" were counted as one-half in computing percentages.)

Pipe	Complete tone		Fundamental eliminated	
	1st pres.	2nd pres.	1st pres.	2nd pres.
Open diapason	4	16	54	54
Violin	38	58	54	69
Tibia	4	4	38	38
Melodia	20	0	58	46
Kinura	58	20	65	84
Reed oboe	12	42	73	77
Flute	8	12	54	42
Open diapason phonon	42	12	73	88
Trumpet	12	8	69	77
Salicional	31	16	80	69

than four) for the octave as the pitch of the complete tone. The results for these *Ss* are presented in Table III.

Table III shows again the tendency the *Ss* had to prefer the octave as the pitch-match of the complete tones in the case of those having a weak fundamental and strong upper partials, *e.g.* the violin, the kinura, and the reed oboe. These *Ss* were more consistent than the whole group in their responses to the tones with strong fundamental, *i.e.* the tibia, the flute, the trumpet, and to a lesser degree the melodia and the salicional. Their response to the open diapason is not so easy to understand, in view of its weak fundamental. Possibly the fact that the series of partials is short tends to make its pitch seem low. Again, it will be seen that when the fundamental is eliminated, a majority of the *Ss* preferred this octave, except again in the case of the tibia.

That musical training and *S*'s certainly about the pitch of tones might be related is conceivable. This relationship is borne out to some extent by the results. The special group of 10 *Ss* contained 7 who had had musical training whereas the group of 26 *Ss* contained, of course, these 7 and only 5 others who were musically trained. There seemed to be no rela-

tion between the amount or kind of training and *S*'s response, except in the case of one *S* who had had considerable experience with the organ, and who was familiar both with the nature of the experiment and with

TABLE III

PERCENTAGES OF 10 *Ss* SELECTING THE OCTAVE OF THE FUNDAMENTAL

(Responses marked with "?" were counted as one-half in computing percentages.)

Pipe	Complete tone		Fundamental eliminated	
	1st pres.	2nd pres.	1st pres.	2nd pres.
Open diapason	0	0	45	60
Violin	20	50	40	75
Tibia	0	0	40	40
Melodia	15	0	65	60
Kinura	40	15	40	80
Reed oboe	10	25	70	60
Flute	0	0	55	60
Open diapason phonon	10	5	95	95
Trumpet	0	0	95	80
Salicional	15	0	50	80

Fletcher's work. His responses were in favor of the pitch of the fundamental in every case where the fundamental was present, and in 15 of the 20 cases where it was absent.

DISCUSSION

That these results are at variance with Fletcher's is evident. Under conditions similar to his, the present *Ss* failed to report unchanged pitch when the fundamental was eliminated from the tones of organ pipes. The nearest approach to Fletcher's results occurred in the case of the tibia, and even here approximately 40% of the *Ss* preferred the octave as the pitch-match.

Several explanations of the discrepancy seem possible. One is that the *Ss* in the two sets of experiments were differently trained. Most people, even the musically educated, are unfamiliar with pure tones, and when asked to compare the pitch of a pure tone with that of a complex tone, may base their judgments on timbre rather than on pitch, and so choose that frequency of the pure tone which is most like the complex tone in timbre. Something of this sort may have been happening in the present experiment when the *Ss* chose the octave as the pitch-match of the complete tone of some of the pipes. This possibility was further borne out in some early experiments where the *Ss* were asked to match the pitch of the complex tone devoid of its fundamental with the tone of a harmonium. Then the *Ss* tended more often than in the present experiment to choose the fundamental.

On the other hand, it may be that pitch is a significant attribute of tone only when the tone has a fundamental strong enough to stand out. Ekdahl and Boring,[6] found that, when *Ss* were asked to judge the pitch of masses of sound, they chose as the pitch of the mass a frequency which was an average of the pitches of the components of the mass. It may be that the present *Ss* were making a judgment of that sort. A person listening to the complex tone of such a pipe as the kinura, especially when its fundamental is eliminated, is inclined to feel that the tone is not 'musical,' that it is more like a chord or noise in quality than like a single tone. Fletcher's *Ss* reported the same experience when the incomplete tone to which they listened was not strong enough to generate an appreciable difference-tone-fundamental, and also when the difference-tone was of a different frequency from the quotient of the partials.

A third possibility, not inconsistent with either of the preceding, is that in the present experiment the ear-generated difference-tone was not strong enough, even though in some cases an intensity of 100 db. was used, to give the tone a definite pitch. This is by no means unlikely. Let us consider the circumstances under which such difference tones arise.

Let us assume that the response of the ear is non-linear and that we may express its displacement, y, as a function of the applied force x, in the form of a power series $y = ax + bx^2 + cx^3 + \ldots$ For purposes of the present discussion we may neglect the terms of higher than the second power, although Newman, Stevens, and Davis[7] and Wever and Bray,[8] have shown that the effects of terms as high as the sixth are detectible in the response of the cat's ear. Now if we apply to the ear three tones of frequency f_1, f_2, and f_3, so that their force amplitudes are equal, and, for convenience, equal to unity, we may write $x = \sin(2\pi f_1 t) + \sin(2\pi f_2 t + \varphi_2) + \sin(2\pi f_3 t + \varphi_3)$, where φ_2 and φ_3 are the differences in phase respectively between 2 and 1, and 3 and 1. If we now substitute this value of x in the first equation and reduce all of the terms to first powers of sines and cosines, we arrive at an expression for y involving such frequencies as f_1, f_2, f_3, $(f_2 - f_1)$, $(f_3 - f_2)$, $(f_1 + f_3)$,

[6] A. G. Ekdahl and E. G. Boring, The pitch of tonal masses, this JOURNAL, 46, 1934, 452-455.

[7] E. B. Newman, S. S. Stevens, and H. Davis, Factors in the production of aural harmonics and combination tones, *J. Acous. Soc. Amer.*, 9, 1937, 107-118.

[8] E. G. Wever and C. W. Bray, The nature of acoustic response: The relation between sound intensity and the magnitude of responses in the cochlea, *J. Exper. Psychol.*, 19, 1936, 129-143; Distortion in the ear as shown by the electrical responses of the cochlea, *J. Acous. Soc. Amer.*, 9, 1938, 227-233.

etc. If we consider only the case where f_1, f_2, and f_3 are multiples of some lower frequency f (*e.g.* $f_1 = 2f$, $f_2 = 3f$, and $f_3 = 4f$), and concern ourselves only with those terms that will yield f, we shall be interested in $(f_2 - f_1)$ and $(f_3 - f_2)$. They will appear in the final result as $bCos\ (2\pi\ (f_2 - f_1)t + \varphi_2)\ +\ bCos\ (2\pi(f_3 - f_2)t + \varphi_3 - \varphi_2)$, and, since $f_2 - f_1 = f_3 - f_2 = f$, we may write this as $2bCos\ (\varphi_2 - \frac{1}{2}\varphi_3)\ Cos\ (2\pi ft + \varphi_3)$.

In this last expression the phase angles φ_2 and φ_3 enter in such a way as to modify the amplitude of the difference-tone, and if $\varphi_2 - \frac{1}{2}\varphi_3 = \pi/2$, the difference-tone due to the square-term disappears. If $\varphi_2 - \frac{1}{2}\varphi_3$ has any value other than zero, this intensity of the difference-tone will be less than maximal. Similar relations, though not so simple, exist for the difference-tones due to the higher-order terms. Thus we see that the phase relations existing among the various partials of a complex tone may play a considerable part in determining the intensity of any difference-tone that may be established.

This fact may be of considerable importance to the problem of pitch. In the tone of musical instruments, the phase relations among the partials will depend, among other things, upon the location of the hearer, or, as in the present experiments, of the microphone, with reference to the instrument. In the case of the synthetic tone employed by Fletcher in many of his experiments, it is likely that the phase angles were all zero, and consequently the difference-tone maximal in intensity.

A further, and probably still more important factor affecting the nature of the difference-tones is the frequency relations among the partials. We have so far assumed, and it seems to be rather generally accepted as fact by many writers on hearing, that the frequencies of the partials of a complex tone are integral multiples of the frequency of the fundamental. This is by no means the case in the tones of most, probably all, musical instruments.[9] In stringed instruments, unless the bridges are perfectly rigid and the string perfectly homogeneous, the partials will deviate slightly from a harmonic series, and in the case of some organ pipes the series may be far from a harmonic one. If we examine the effect of this fact upon our results above, we see that it will be to make $(f_2 - f_1)$ different from $(f_3 - f_2)$. Thus we will have two difference-tones differing slightly in frequency and beating together, with a consequent reduction in the contribution they make to the pitch-definiteness of the com-

[9] See, for example, any treatise on sound, such as P. M. Morse, *Vibration and Sound*, 1936, chaps. 3 and 6.

posite tone. Here again Fletcher's complex-tone generator is an exception. The partials it produces are integral multiples of its fundamental frequency and so would generate in the ear a difference-tone (of the order we have been considering) having only a single frequency.

The most plausible explanation of the difference between the results of the present series of experiments and Fletcher's seems to the writer to involve all of the above considerations; namely, that the average *S*, even the average musically trained *S*, finds difficulty in comparing the pitch of a complex tone with that of a pure one, especially if the complex tone has a weak fundamental and a series of strong higher partials; that practice *may* improve this sort of judgment; that, in the present series of experiments, the difference-tones generated in the ear were, for various physical reasons, weaker than those in many of Fletcher's experiments; and, finally, that Fletcher's tone generator (with which his most striking results were obtained) was especially apt to produce the ear-generated fundamental frequency.

Conclusions

(1) *S*s attempting to identify the pitch of organ-pipe tones lacking the fundamental component find great difficulty in doing so, and, in most cases, choose the octave of the fundamental-frequency as the pitch of the tone. Exceptions may occur in the case of tones whose partials are so related as to favor the generation of strong difference-tones in the ear.

(2) The foregoing experiments failed to provide any evidence in favor of the hypothesis that pitch is determined by the nature of the pattern of stimulation of the basilar membrane, or that the phenomenon of pitch-stability is analogous to pattern-completion in vision.

MULTIPLE FACTORIAL ANALYSIS OF FINE MOTOR SKILLS

By R. H. SEASHORE, C. E. BUXTON, Northwestern University, and
I. N. MCCOLLOM, Colorado State College of Education

The experiment to be reported was undertaken as an extension and combination of numerous earlier experiments on the nature of motor abilities.[1] It has been shown that motor abilities are only slightly though positively correlated, on the average, but questions have been raised as to the adequacy of the experimental conditions under which such results have been obtained, and as to the reliabilities of the tests employed. Our first procedure was simply to improve and lengthen these tests until such difficulties were overcome. Having verified, by our zero-order correlations in Table II, the absence of any one general factor of motor ability, our major problem became that of locating and identifying any possible group factors among the performances.

This study brought together for the first time tests of simple reaction-time, simple tapping rate, complex tapping rate, serial high speed co-ordinations, postural steadiness, and form board and assembly tests of mechanical abilities.

DESCRIPTION OF TESTS

In the following descriptions of the tests employed we shall first describe the characteristics of each group of tests and then the distinguishing features of each specific test. To avoid undue repetition we shall present the data as briefly as possible in a standard order, giving the number and the name of the tests, the nature of the performance, the type of scoring, the length of each trial, the number of trials per cycle, and the number of cycles.

Group A. Simple reaction-times. Group A includes Tests 1–4. In all of these tests a new silent chronoscope was employed which measured reactions in units of 1/100 sec.[2] Fifty trials were given for each test on each of two cycles.

Test 1. Auditory simple reaction. *S* depresses a telegraph key 1 mm. in response to the click of a telegraph sounder.

Test 2. Visual simple reaction. Same response to flash of light.

Test 3. Auditory jump reaction. *S* moves hand 6 in. horizontally to depress key in response to click.

Test 4. Visual jump reaction. Same movement as Test 3, to light flash.

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[1] R. H. Seashore (An experimental and theoretical analysis of fine motor skills, this JOURNAL, 53, 1940, 86-98), has reviewed the history, theoretical bases, and minor experiments on this problem.

[2] This chronoscope was devised by R. H. and S. H. Seashore.

Group B. Tapping and related actions. Group B includes Tests 5–13, all of which have been described by McCollom.[3] All of these tests, except Test 13, employed a highly accurate timing device to record the number of taps in 5-sec. intervals, as registered by a Stoelting Hollerith counter which would record up to 12 taps per sec.

Test 5. Simple vertical tapping. *S* operates a vertical telegraph key having an excursion of 1 mm. and a spring resistance of 100 gm., by grasping the key, and by using free arm movement. Five trials were given for each of 3 cycles.

Test 6. Same as Test 5, but given two weeks earlier.

Test 7. Simple horizontal tapping. Same as Tests 5 and 6 except movement is horizontal instead of vertical.

Test 8. One-plate stylus tapping. *S* taps pencil shaped stylus vertically on metal plate 2½ in. sq. Three trials each on 3 cycles for Tests 8, 9, and 10.

Test 9. Two-plate stylus tapping. *S* taps same stylus alternately on two plates; 4½ in. from center to center.

Test 10. Three-plate stylus tapping. *S* taps same stylus on three plates in triangular pattern.

Test 11. Oscillometer, two finger. *S* grasps a fixed vertical rod with thumb, 4th and 5th fingers, using 2nd and 3rd free fingers to move a horizontal lever which makes and breaks an electrical contact to the counter for each change in direction of movement. Amplitude uncontrolled. Three trials for each of 3 cycles for Tests 11 and 12.

Test 12. Oscillometer, arm. Same as Test 11, except whole arm is free to oscillate.

Test 13. Spool-peg transfer. *S* picks up spool from one row of pegs, transfers it to corresponding position in second row. Move[illegible] are designed to involve simple grasping and transport movements equivalent [illegible]ests 11 and 9. Twelve pegs per row, averaging approximately 12 sec. per trial[illegible] trials for each of 3 cycles.

Group C. Stanford motor skills unit. Gr[illegible] C, besides the following tests, includes Test 5.[4]

Test 14. Spool-packing, Seashore-Tinker apparatus. *S* grasps one spool in each hand, packing in pairs in a tray. The number of trays packed in 1½ min. is taken as *S*'s record. Two trials for each of 3 cycles.

Test 15. Speed drill (Miles). *S* turns Goodell-Pratt hand drill with arm at right angle to wheel—a rotary wrist flipping movement. Veeder counter measures the number of revolutions in 10 sec. Three trials in each of 3 cycles.

Test 16. Serial discrimination. Numbers 1 to 4 appear in random order at window, *S* presses corresponding one of four keys with fingers of right hand. The number of correct discriminations is indicated in high-speed escapement apparatus during 2-min. trials. Two trials per each of 3 cycles.

Test 17. Pursuit-rotor (Koerth). A brass target ½ in. in diam. is embedded near the edge of a revolving fiber disk on phonograph turntable. *S* attempts to keep a hinged (pressureless) pointer on the target moving at 60 r.p.m. Score is the number of twentieths of a revolution that *S* holds the pointer on the target, as measured by electrical commutator and counter. Twenty revolutions per trial; 10 trials per each of 3 cycles.

Group D. Postural sway. Group D includes two tests, Tests 18 and 19.

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[3] I. N. McCollom, An analysis of factors determining individual differences in speed of simple repetitive motions of the right arm (unpublished Master's thesis, Univ. of Oregon Library, 1932).

[4] R. H. Seashore, Stanford motor skills unit, *Psychol. Monog.*, 39, 1928, (no. 178), 51-66.

TABLE I

ZERO-ORDER CORRELATION AMONG THE VARIOUS TESTS

No.	Test	Test (number)																			
		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1.	Aud. simple reac. time	.55	.44	.51	.09	.25	.28	.05	−.01	.05	.19	.15	−.14	−.15	.23	.03	−.60	−.18	−.24	−.10	−.03
2.	Vis. simple reac. time		.49	.87	.18	.36	.44	.22	−.30	.01	.23	.27	.10	−.21	.10	.04	−.05	.06	−.02	.04	.10
3.	Aud. jump reac. time			.78	.36	.41	.36	.49	.17	.02	.18	.26	.44	.06	.24	.36	.14	−.04	.29	.10	−.12
4.	Vis. jump reac. time				.29	.38	.44	.44	.21	.18	.29	.34	.22	.12	.18	.16	.14	−.10	.18	.11	−.04
5.	Vert. tel. key tap					.89	.74	.59	.36	.25	.61	.65	.35	.02	−.33	.18	.21	.31	−.13	−.08	−.10
6.	Same (2 wk. earlier)						.93	.79	.06	.33	.71	.87	.32	−.02	.18	.23	−.25	−.15	−.03	−.14	−.54
7.	Hor. tel. key tap							.63	.40	.23	.72	.66	.31	.20	.23	.17	.30	−.18	−.17	−.21	−.30
8.	One-plate stylus tap								.49	.31	.59	.39	.44	.12	.28	.47	.03	−.15	.04	−.19	−.29
9.	Two-plate stylus tap									.79	.45	.51	.66	.17	.32	.34	.43	−.08	.06	.27	−.07
10.	Three-plate stylus tap										.44	.39	.65	.26	.15	.77	.24	.27	−.03	.22	−.21
11.	Oscillometer, 2-finger											.78	.13	.03	.10	−.10	−.01	.06	.22	−.21	−.23
12.	Oscillometer, arm												.44	−.18	.22	.20	.11	−.14	−.21	−.16	−.08
13.	Spool-peg transfer													.19	.25	.30	.23	−.28	−.16	.31	.14
14.	Spool-packing														.10	.14	.04	−.21	−.01	.32	−.21
15.	Speed drill															.09	.10	−.24	−.27	−.12	−.13
16.	Serial discrimeter																.17	−.25	.08	.16	.00
17.	Pursuit-rotor																	−.62	−.37	.42	.41
18.	Ataxiameter, stand																		.57	−.28	−.20
19.	Ataxiameter, sit.																			−.03	−.16
20.	Spatial relations																				.51
21.	Assembly																				—

Test 18. Ataxiameter,[8] standing. *S* stands erect, heels together, toes at angle of 45°, eyes closed, as still as possible. Light weight ratchet counters measure total amount of movement in millimeters in front, back, right, and left directions. Trials

TABLE II

INTERCORRELATIONS FOR MOTOR SKILLS TESTS OBTAINED IN THIS AND OTHER STUDIES

(Unmarked coefficients are for the present study; others obtained from studies indicated below.)

Test	Test						
	16	5 & 6	14	15	18	10	1–2
17 Pursuit-rotor	.25 (2)	.31 (2)	.26 (2)	.38 (6)	.12 (2)	.06 (6)	.15 (1)
	.17 (3)	.01 (3)	.03 (3)	.33 (2)	.62	.25	.14
	.29 (4)	.28 (4)	.44 (4)	.16 (3)	.37		.23
	.17	.25 (5)	.32 (5)	.40 (4)			−.05
		.21	.13 (6)	.10			.62
		−.25	.04				
16 Serial Discrimination			.32 (2)	.08 (2)	−.15 (2)	.77	.36
		.03 (3)	.29 (3)	.24 (3)	.24		.16
		.19 (4)	.34 (4)	.06 (4)	−.08		.03
		.18	.14	.09			.04
5 Telegraph-key tapping				.48 (3)			.10 (1)
			.36 (3)	.06 (4)			.35
			.35 (4)	−.35			.29
			.40 (5)				.09
			.02				.18
14 Spool-packing				.34 (6)	.15 (2)	−.38 (6)	.06
				.38 (2)	.20		.12
				.29 (3)	.01		−.15
				.43 (4)			−.21
				.10			
15 Speed drill					.12 (2)	.52(6)	.24
					.24	.26	.18
					.27		.23
							.10

(1) R. H. Seashore, Studies in motor rhythm, *Psychol. Monog.*, 36, 1926, (no. 167) 142–189 (44 college men).
(2) R. H. Seashore, Individual differences in motor skills, *J. Gen. Psychol.*, 3, 1930, 38–65 (50 university men; 35 undergrad., 15 grad.).
(3) S. H. Seashore, The aptitude hypothesis in motor skills, *J. Exper. Psychol.*, 14, 1932, 555–561 (50 knitting mill workers, ages 19–26 yr.).
(4) R. Y. Walker and R. D. Adams, Motor skills: The validity of serial motor tests for predicting typewriting proficiency, *J. Gen. Psychol.*, 11, 1934, 173–186 (70 high school boys, ages 15–18 yr.).
(5) C. E. Buxton and L. G. Humphreys, The effect of practice upon intercorrelations in motor skills, *Science*, 81, 1935, (no. 2105), 441–442 (50 college men, 4 years range in class).
(6) C. E. Buxton, The application of multiple factorial methods to the study of motor abilities, *Psychomet.*, 3, 1938, 85–93 (76 high school boys, ages 12–16).

2 min. each, 4 trials per cycle, one cycle before and one after Stanford motor skills unit tests of Group C.

[8] W. R. Miles, Static equilibrium as a useful test of motor control, *J. Indus. Hyg.*, 3, 1922, 316-331.

Test 19. Ataxiameter, sitting. Same as Test 18 except *S* sits on backless stool.

Group E. Minnesota mechanical ability tests.[6] Group E includes Tests 20 and 21.

Test 20. Form Boards A, B, C (spatial relations). *S* fits diverse shaped wooden blocks into holes by standard method.

Test 21. Assembly A, B, C. *S* assembles simple mechanical gadgets in fixed times. Score is number assembled under standard method.

Fifty men, working as a part of a regular psychology laboratory course, were tested individually with standard instructions, demonstrations by the experimenter, and criticisms of their techniques as previously used by Seashore and others.

The cycle plan was used, whereby the *S*s worked approximately 5 minutes on each test in rotation, the testing being distributed over 3 days. Scoring was done by

TABLE III

CORRELATIONS R_{pq} BETWEEN PRIMARY AXES IN THE EXPERIMENTAL POPULATION

(The residual plane VI was set orthogonal to the other planes in determining these correlations.)

	I	II	III	IV	V
I	1.00	.27	−.04	−.28	.30
II	.22	1.00	.21	−.02	.41
III	−.04	.21	1.00	−.04	.08
IV	−.28	−.02	−.04	1.00	.19
V	.30	.41	.08	.19	1.00

automatic devices, with immediate knowledge of results. The *S*s' knowledge of their scores, together with the intrinsically interesting character of the tests, provided adequate and reasonably constant motivation. This motivation and the careful control of the testing produced, in the main, highly reliable tests (see Table III). In Table I we have reproduced our own table of zero-order correlations so that later workers may use the data in different ways and compare the r of their studies with previous studies as we have done in Table II.

In this attempt to discover any statistically significant grouping of tests, Thurstone's center of gravity method of analysis was employed. Orthogonal axes were used originally, and rotation was done by the standard technique.[7]

Table IiI gives the correlations between the primary axes, but the few correlations above three P.E. of their values do not suggest any readily apparent qualitative basis. These values must be verified on larger test populations before they may be assumed to have any definite significance.

Since the battery included groups of most of the known varieties of tests of fine motor skills, the data seemed to afford a legitimate opportunity for the identification of possible group factors by the Thurstone method.

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[6] D. G. Paterson, L. D. Anderson, H. A. Toops, and E. Heidbreder, *Minnesota Mechanical Ability Tests*, 1930, 1-586.

[7] L. L. Thurstone, *The Vectors of Mind*, 1935, 1-266. The writers are greatly indebted to Mr. Harold Bechtoldt who suggested and kindly provided a revised factorial matrix table based upon the assumption of oblique rather than orthogonal axes. This change cleared up a number of minor anomalies which had arisen from the orthogonal treatment and indicated that the data did show a better fit to the assumption of oblique axes. Mr. Bechtoldt also provided Table III.

Results and Conclusions

Table IV shows that 6 factors were necessary to account for the zero-order table of correlations. For convenience in inspection of this table loadings of 0.40 or higher were arbitrarily considered as being of probable significance and are printed in boldface. Some of the loadings are starred to indicate that in this preliminary survey they disagree somewhat with our qualitative identifications.

From previous studies we should expect that a group-factor among motor tests would appear in performances involving a *similar pattern*

TABLE IV

Rotated Factorial Matrix, Giving Test Projections on the Six Oblique Axes
(Figures in boldface are probably significant.)

Test	‡$r_{\frac{1}{2}\frac{1}{2}}$	I	II	III	IV	V	VI	h^2†	h^2_{assum}
1 Aud. simple reac. time	.63	**.65**	.01	.03	−.03	−.06	−.10	.54	.60
2 Vis. simple reac. time	.90	**.64**	.15	.08	−.06	.15	.16	.82	.87
3 Aud. jump reac. time	.74	**.48**	.20	.47	−.03	−.09	−.05	.66	.78
4 Vis. jump reac. time	.86	**.75**	.32	.09	.02	−.02	.01	.95	.87
5 Vert. tel. key tap.	.92	.03	**.89**	.04	.02	−.51*	−.03	.91	.89
6 Same (2 wk. earlier)	.85	−.10	**.86**	−.10	−.08	−.05	−.39	1.09*	.93
7 Hor. tel. key tap.	.91	.56	**.51**	.04	**.74***	**.67***	−.10	.93	.93
8 One-plate stylus tap.	.89	.02	.22	**.40**	.12	**.38**	.18	.64	.79
9 Two-plate stylus tap.	.90	−.04	.31	**.64**	.29	−.06	.11	.73	.79
10 Three-plate stylus tap.	.87	−.10	.38	**.69**	−.10	−.06	−.10	.96	.79
11 Oscillometer, 2 finger	.92	.03	**.70**	−.03	.25	−.01	.05	.68	.78
12 Oscillometer, arm	.85	.03	**.62**	.21	**.45**	−.02	.30	.83	.87
13 Spool-peg transfer	.89	.10	.37	**.43**	.32	.06	.11	.61	.66
14 Spool-packing	.90	.01	.02	−.03	.12	.13	−**.56**	.39	.32
15 Speed drill	.81	**.54***	−.03	.26	.34	−.01	.20	.44	.33
16 Serial discrimeter	.93	.07	−.04	**.69***	−.06	+.06	.00	.58	.77
17 Pursuit-rotor	.92	−.02	.09	.19	**.44***	**.44***	.23	.74	.62
18 Ataxiameter, stand	.80	−.04	.09	−.09	**.75**	.05	−.08	.75	.62
19 Ataxiameter, sit	.89	.27	−.05	−.02	**.67**	−.06	−.03	.48	.57
20 Spatial relations	.82	.02	.03	.04	.01	**.58**	−.11	.53	.51
21 Assembly	.52	.00	−.08	−.01	−.03	**.53**	.37	.52	.54

I=Speed of single reaction; II=Finger-hand speed in restricted oscillatory movements; III=Forearm and hand speed in oscillatory movements of moderate extent; IV=Steadiness; V=Manipulation of spatial relations; VI=Unidentifiable or residual factors.

* Loadings seemingly inconsistent with identifications.

† The values in h^2 and h^2 assumed are taken from the orthogonal solution.

‡ The reliability coefficients, or correlation between the last two cycles of each test, are uncorrected for length.

of action, and that loadings would vary only slightly according to the particular sense field or musculature employed.

Thus we find that Factor I has its most significant loadings in the four simple reaction-times (Tests 1–4), whether the stimulus was visual or auditory, and for both a simple one millimeter press reaction of the finger and hand, or a much longer (6 in.) jump-and-press reaction of the hand and forearm. It is tentatively designated "speed of single reaction." The only other significant loadings in this column are for Test 7, which behaves anomalously in relation to several other axes, and the speed drill (variable 15), which is a test of ability to move the hand continuously in a circle, and, therefore, does not seem to resemble the simple reaction-time tests. Only a retest can check the significance of this latter relationship. Each of our factors shows one or more such anomalies, which may possibly be due to statistical fluctu-

ations from our relatively small sample of 50 cases. In a number of these instances the loadings are definitely known not to be representative of the correlational findings from previous experiments with the same tests, as may be seen in Table II—a summary of intercorrelations reported in previous studies between various pairs of tests which also appear in our battery. It is hoped that such apparent inconsistencies in the present study may be cleared up in future studies planned specifically for this purpose.

In Factor II the significant loadings fall on five tests. Tests 5 and 6, tapping a telegraph key vertically (the same test was given two weeks in succession, which accounts for two entries in the factor table); Test 7, tapping a telegraph key horizontally; Test 11, the two-finger oscillometer; and Test 12, the arm and hand oscillometer. All involve short oscillating movements in one plane only. Test 8, one plate stylus-tapping, fails to receive as high loadings as would be expected in line with our tentative identification of this factor, which is: "finger, hand, and forearm speed in restricted oscillatory movement." Note that the repetitive action tests which call for *wider* movement in two planes do not have high loadings in this factor.

Factor III shows relatively significant loadings on all three stylus-tapping tests (variables 8, 9, 10), with the two- and three-plate stylus-tapping tests receiving definitely heavier loadings. The spool-peg transfer test (Test 13), designed to involve the same distance and accuracy of movements as the two- and three-plate tests, also receives a significant weight. The ability called for seems to be one for "forearm and hand speed in oscillatory movements of moderate extent." These movements are nearly all in two planes, as contrasted with the single plane in Factor II. Two anomalous loadings are found, a moderately large one for the auditory jump reaction-time (Test 3) and a higher one for serial discriminative action of the four fingers (Test 16). The possibility seems to be rather great that the serial discrimination test would, in a different battery, have its most significant loadings in some other factor, but this hypothesis must be checked by further experimentation.

Factor IV is best identified from the two high loadings on the standing and sitting postural sway tests (Tests 18 and 19) and, on the basis of previous experiments, by Seashore and Adams,[8] Buxton and Humphreys,[9] and Buxton,[10] seems to be sampling a group factor which we have found to include a number of performances emphasizing steadiness or precision. It may also be related to a relaxed condition in taking the tests. The weighting of the pursuit-test (Test 17) seems logically to be acceptable but is negated by the correlational results of two previous studies (cf. Table II). Test 7, horizontal telegraph key tapping, is again an anomaly.

In Factor V the weightings of the Minnesota spatial relations form-board and assembly tests, together with the loading of the pursuit-rotor (Tests 20, 21, and 17 respectively), suggest that this factor should be designated "skill in manipu-

[8] R. H. Seashore and R. D. Adams, The measurement of steadiness: A new apparatus and results on marksmanship, *Science*, 78, 1933, (no. 2022), 285.

[9] C. E. Buxton and L. G. Humphreys, The effect of practice upon intercorrelations in motor skills, *Science*, 81, 1935, (no. 2105), 441-442.

[10] C. E. Buxton, The application of multiple factorial methods to the study of motor abilities, *Psychometrika*, 3, 1938, 85-93.

lating spatial relations." This rather complex identification seems necessary because of the possibility that future experiments will show this to be largely a space perception factor. The two negative loadings, of the vertical and horizontal telegraph key tapping tests (Tests 5 and 7) probably have to be explained again in terms of the error in our relatively small group of *Ss*, since no other tapping tests receive significant weights. (So far as we can ascertain by checks, the discrepancies between the loadings of the various telegraph-key tapping tests are not due to computational error.)

The discussions contained in the papers of this series have shown that a modification of the type of interpretation given to motor factors by Buxton may be necessary. The shift probably should be one from an anatomical emphasis to a functional emphasis in the qualitative identification of factors.

Factor VI seems to be a residual for this battery of tests.

Summary

The factors isolated in this study are found to correspond more to *qualitative similarities in the pattern of action,* including perceptual activity, involved in various tests rather than to anatomical units such as the dominant sense-field or even the musculature employed. In keeping with previous experiments as summarized by Seashore,[11] we find that shifting from one musculature to another makes very little relative difference in performance, while keeping the musculature constant and shifting to another pattern of action makes a great deal of difference in the correlations. With the exception of Factor IV (steadiness), our factors include such narrow groupings of performances as to make application to the prediction of complex practical skills a hazardous undertaking, a result which is corroborated by our industrial experiments to date. The more exact boundaries of the factors which we have located can only be determined by the verification of the doubtful loadings in this experiment, plus an extension of the battery of tests into those which grade off qualitatively from those now receiving significant weights.

In view of the findings of this and previous studies as to the significance of patterns of action, rather than any basic biological constants of sense field or musculatures, the next logical step in identifying these statistically located factors would seem to be the employment of the techniques of photographic motion study and simple introspection by the *Ss*, rather than the all-too-frequent second hand analysis or vicarious introspections of the experimenter.

We need not deny the existence of individual differences in biological factors, such as speed of muscle contraction, but, as might be inferred

[11] R. H. Seashore, An experimental and theoretical analysis of fine motor skills, this Journal, 53, 1940, 86-98.

from the study of Bryan and Harter on higher units in telegraphy,[12] and the similar studies of Book on typing,[13] *any one physiological limit is usually important only for a given work method and changing to another work method may either partially or entirely overcome this limitation.* Further factor analyses in the motor field will need perhaps more to be supplemented by qualitative techniques, such as motion study of these methods, than by improvement of statistical techniques.

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[12] W. L. Bryan and Noble Harter, Studies in the telegraphic language: The acquisition of a hierarchy of habits, *Psychol. Rev.*, 6, 1899, 374-385.
[13] W. F. Book, *Learning to Typewrite*, 1925, 120-122.

OCULAR PATTERNS AND THEIR PSYCHOLOGICAL IMPLICATIONS

By HERMAN F. BRANDT, Drake University

A majority of the studies of eye movement have been made in the field of reading. The reasons for this are: (1) the importance attached to the diagnosis of reading; (2) a lack of adequate laboratory equipment for other types of research; and (3) the inability to see the possibilities of studying the learning process by means of the analysis of ocular patterns.

Cameras used in photographing eye movements during reading have for the most part recorded eye movements in the horizontal plane only. While this has been satisfactory as a means of revealing certain phases of the reading process, problematic materials, advertising and pictorial copy of necessity require records of the horizontal and vertical movement at all times. In order to obtain the necessary patterns for this study a bidimensional camera was employed.[1]

Among the studies of eye movements (other than those resulting while the *S* was reading) are those made while *S* was looking at pictures,[2] studying romance languages,[3] learning algebraic and chemical formulas,[4] reading music,[5] and taking examinations.[6] In every case, new information has been revealed leading to the general conclusion that eye movements have a functional relation to the central processes of the learner.

Dallenbach in his research on position and intensity as determinants of attention, concludes that spatial position was an important factor and that other variables such as the relative intensity of light was affected by it.[7] In his experiments, *O*, at a given signal, fixated a spot of light.[8] Then two diametrically opposed areas of equal size and of equal distance from the fixation-point were simultaneously exposed. *O* reported which was the clearer. The results obtained indicated that the positions to the left and above have a significant attentional advantage.

In a later article, Burke and Dallenbach, using the same apparatus, substantiate

* Accepted for publication May 3, 1939.

[1] H. F. Brandt, A bidimensional eye-movement camera, this JOURNAL, 49, 1937, 666-670.

[2] C. T. Buswell, *How People Look at Pictures*, 1935, 1-198.

[3] W. R. Miles and E. Shen, Photographic recording of eye movements in reading of Chinese in vertical and horizontal axes, *J. Exper. Psychol.*, 8, 1925, 344-362.

[4] M. A. Tinker, A photographic study of eye movements in reading formulas, *Genet. Psychol. Monog.*, 3, 1928, 65-182.

[5] O. Jacobson, An analysis of eye movements in reading music, Univ. Chicago, 1926 (unpublished thesis).

[6] Arden Frandsen, An eye movement study of objective examination questions, *Genet. Psychol. Monog.* 16, 1934, 85-138.

[7] K. M. Dallenbach, Position *vs.* intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286.

[8] Dallenbach, Some new apparatus, *ibid.*, 34, 1923, 90-95.

their previous findings and add that position seems to be a matter of left and right handedness; and that the position to the left and above the center has a distinct attention value for the right handed while the position to the right and below has an attention advantage for the left handed.[9] A neurological explanation of right and left dominance was offered by Dallenbach.[10] Papez and Cohu accepted it. They state that "neural excitations aroused in the right retinas of each eye—the areas stimulated by objects occupying positions to the left of fixation—center in the right hemisphere of the brain where they, because right hand people are left brained, are comparatively free from interference and inhibition by other concomitant processes. As a consequence, they possess on the side of consciousness a high degree of attensity, *i.e.* clearness or vividness. Everything else being equal, objects arousing processes in the right brain would enjoy therefore, an attentional advantage over all others."[11]

Nixon,[12] employing the visual method for measuring the attention-value of advertisements, tested his *Ss* by having them look at a pair of advertisements for 30 sec. The direction of the first eye movements and the relative amount of time spent in the right and left position, were observed by *E* from behind a screen. A recall test was used to check the results. Nixon concludes on the basis of his experiment that there is a marked tendency for the first fixation to fall on the left hand page. This tendency, he found, falls off rapidly with the passing of time, but a superior memory value is apparent for the page receiving the greatest primary attention.

Guilford and Hackman developed another method of measuring the attention-value of stimuli.[13] Their method was more objective in that they employed a camera to record *S*'s eye movements and insisted on immediate recall as over against Nixon's delayed response. The stimulus-material for their experiment consisted of lantern slides, made up by typing letters or gluing geometric forms on radio mats. Various slides were used representing accepted conditions or factors of attention such as: position, isolation, size, and novelty. The above slides were exposed in a tachistoscopic arrangement by means of a projection lantern and recorded by an eye movement camera. The direction of the first eye movement was noted and also the time spent to the right and to the left of the fixation-point. In another series of slides, eye movements were recorded while *S* looked at the exposure-card for 10 sec. They draw the following conclusions: (1) the relative time spent in looking at the right or left is about the same; (2) the length of time spent on right or left is not a good indicator of the attention-value of the side of the field containing the factor of attention; (3) the direction of the first eye movement rather than the length of the fixation is a somewhat better indicator that there is an attention gaining factor present; and (4) a higher correlation exists between eye movements and cognition than between eye movements and attensity.

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[9] Ruth Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left handed observers, *ibid.*, 35, 1924, 267-268.

[10] Dallenbach, *op. cit.*, 285 f.

[11] H. A. Cohu and J. W. Papez, A comparative study of the visuosensory or striate area in the two hemispheres of the human brain, *Amer. J. Phys. Anthrop.*, 14, 1930, 413 f.

[12] H. K. Nixon, Attention and interest in advertising, *Arch. Psychol.*, 11, 1924, (no. 72), 1-68.

[13] J. P. Guilford and H. B. Hackman, A study of visual fixation-method of measuring attention value, *J. Appl. Psychol.*, 20, 1936, 44-59.

PURPOSE

The purpose of the present study is to analyze ocular patterns while the *Ss* are observing a symmetrical field. It is an attempt to discover characteristic eye movements and their psychological implications. In seeking an answer to these objectives, four problems present themselves. (1) Has the eye a preference for the right or left side of the page? (2) Is the upper or lower half of the page preferred? (3) Does the eye prefer horizontal or vertical excursions? (4) Is the excursion distance greater in the horizontal or vertical plane?

This study also endeavors to show that these ocular patterns reveal certain physiological and psychological implications.

METHOD AND PROCEDURE

Subjects and instruction. The *Ss*, 50 college students (29 men and 21 women), were chosen at random. Preceding the presentation of the exposure-material, every *S* was instructed regarding his task. He was told to observe the exposure-card,[14] to study the pattern as he would later be called upon to reproduce it.[15] He was not, however, informed of the length of the exposure-time.

Exposure-card. The exposure-material, a white card cross-ruled into 25 equal areas each 2 in. sq., is shown in Fig. 1. Every cross-ruled division contained from 1 to 9 small black squares each ¼ in. sq. These were symmetrically arranged around the center area which contained one black square. Proceeding outward from the center area the number of small black squares increased uniformly in every direction. It is evident that no matter which area is first observed, another area containing the same number and arrangement of black squares may be found in another position. There should be no pattern-preference in such a uniform field.[16]

Fixation: (a) Duration. After being developed, each film with the ocular movements for that particular person was placed on a calibrated, illuminated, opal flash glass for the purpose of identifying and coding each fixation. The duration of each fixation was determined by measuring the distance the film had traveled.[17]

(b) Location. After all fixations of both eyes had been identified and coded, the film was placed in a high-power stereopticon and projected on a field calibrated to locate each fixation on the card originally observed. The calibration in both horizontal and vertical dimensions provided an area on which every point could be designated. If, for example, the fixation on the record fell at a point 75% of the horizontal distance from the left side and 25% of the vertical distance from the base, the resultant would be a point in the lower right hand corner where the eye fixated at

[14] Every *S* was shown the exposure-card for 20 sec.
[15] Immediately after the exposure, *S* was given a blank duplicate of the exposure-card with the instruction that he was to reproduce the original pattern.
[16] The size of the original exposure card as observed by the *Ss* was 10 x 10 in.
[17] The mean duration of the fixations for all *Ss* was 0.275 of a sec.

the time of observation. Consequently every fixation was designated by two co-ordinate numbers constituting a definite point in the area.

Excursion: (a) Distance. The points established were next plotted for the entire period and the distance between subsequent points taken as a measure for determining the distance and direction of the excursions. The excursion from the first to the second fixation was considered the first excursion.

(b) Direction. The grid for the location of eye fixations was placed under tracing paper upon which the ocular pattern was recorded. The excursion directions were

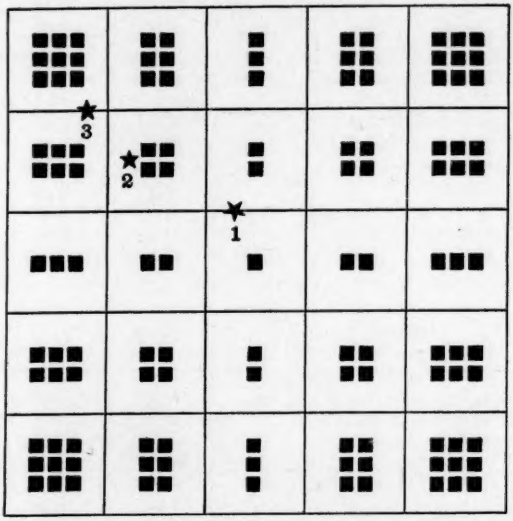

FIG. 1. MEDIAN LOCATION OF FIRST, SECOND AND THIRD FIXATION OF ALL THE *Ss* ON THE EXPOSURE-CARD

designated either as horizontal or vertical. If the line formed an angle greater than 45° and less than 135° with the horizontal, the excursion-line was said to be vertical. All other excursion-lines were classified as horizontal.

RESULTS

The median for the first fixations of all *Ss*, as indicated in Fig. 1, was located at a point above and to the left of the center of the card. This point is generally known as the optical center. The median of all the second fixations indicated that the eye had a tendency to move up and toward the left while the median of the third fixations was located still closer to the upper left hand corner.

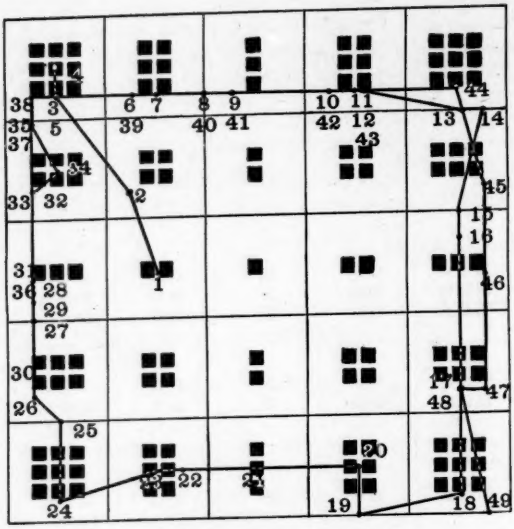

FIG. 2. OCULAR PATTERN OF *S* 14
(Achievement score 40 out of a possible 100.)

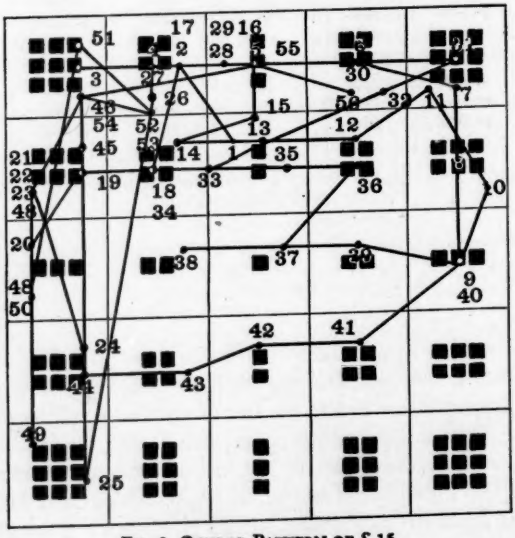

FIG. 3. OCULAR PATTERN OF *S* 15
(Achievement score 100.)

While all conclusions are based upon the performance of 50 *Ss*, the results of only two *Ss* are given here to illustrate the characteristic ocular patterns. The pattern given in Fig. 2 shows that *S* 14 did not see the cue relations. He returned answers only in the area where fixations were made.[18] Ten areas were correctly reproduced.

S 15 evidences in Fig. 3 an ocular pattern adequate for his purpose of reproducing the original pattern perfectly. From all indications, this *S* recognized the cue to the problem and discovered the existing relationships.

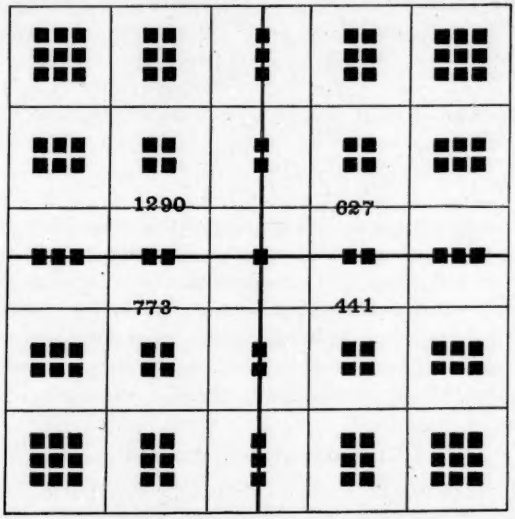

FIG. 4. RELATIVE FIXATION-FREQUENCIES IN AREAS OF A SYMMETRICAL FIELD

It appears that if the correct relationships are perceived, it is unnecessary to make complete coverage in order to reproduce the card. Evidently parts not seen may be reproduced when based on assumed relations.

The distribution of fixations as indicated by the above figure, implies a strong preference for the upper left hand area (41%) followed by the lower left (25%) and the upper right (20%) respectively. The lower right (14%) receives the least number of fixations of all four areas.

[18] The numbers in Figs. 2 and 3 represent fixation-points as recorded by the camera. Number 1 indicates the first fixation with subsequent fixations following in numerical order. The line from fixation 1 to fixation 2 and subsequent fixations represents an excursion.

The average frequency of fixation in the different parts of the visual field and the average frequency and length of the horizontal and vertical excursions for 50 *Ss* are given in Table I. These data serve as answers to

TABLE I

RELATIVE FREQUENCY OF FIXATION IN THE DIFFERENT SECTIONS OF THE VISUAL FIELD, AND THE RELATIVE FREQUENCY AND DISTANCE (IN CM.) OF THE HORIZONTAL AND VERTICAL EXCURSIONS

Position	Mean	$S.E._M$	$M_{diff.}$	$S.E._{Mdiff.}$	CR
Left (freq.)	41.86	1.87	21.74	2.15	9.35
Right (freq.)	20.12	1.61			
Upper (freq.)	40.62	1.43	17.64	2.58	6.82
Lower (freq.)	22.98	1.85			
Horizontal (freq.)	35.34	1.6	14.38	.848	17.0
Vertical (freq.)	20.96	3.4			
Horizontal (dis.)	1955.98	102.1	862.93	16.31	52.8
Vertical (dis.)	1093.05	86.1			

the four questions raised at the beginning of this study. They reveal (1) that the left side of the field is fixated more frequently than the right; (2) that the upper half of the field is fixated more frequently than the lower; (3) that the horizontal excursion occurs more frequently than the vertical; and (4) that the sweep of the horizontal excursions greatly exceeds that of the vertical. In all of these cases, as Table I shows, the critical ratios (CR) are greater than 3.0 and the results are, therefore, statistically reliable and significant.

SUMMARY AND CONCLUSIONS

The purpose of this study was to discover by means of ocular photography, characteristic eye movements and their psychological implications. Among the most outstanding characteristics of the ocular patterns were the frequency and location of fixations as well as direction and distance of excursions. Frequency and duration of fixations constitute measures of protensity, while excursion distance and direction imply extensity. Both are a type of dimension which may be measured with laboratory instruments. From the analysis of the ocular patterns it is apparent that factors other than variables in the content are responsible for certain modes of behavior.

The results of this investigation are: (1) the mean of the initial fixation is located at a point to the left and above the center of the observed field; (2) the second and third fixations are located to the left and above the first; (3) the fixations on the left half of the visual field exceed those on

the right; (4) the upper half of the page is preferred to the lower; and (5) there is a strong tendency toward the facilitation of horizontal and the inhibition of vertical movements, thus affecting the excursion distance and direction.

The general conclusion to be drawn from these results; namely, that position is an important determinant of attention, is corroborated by earlier investigators. The slight variances discovered are largely due to the type of content selected for observation and the techniques employed in the respective experiments. Results are, for that reason, not always comparable.

Dallenbach and his associates agree with the general hypothesis that position is a potent factor in determining attention-value, and in addition support the conclusions that the left and top positions of the observed field have significant attentional advantage. Nixon, based upon his investigation, concludes that *Ss* observing a pair of advertisements for 30 sec. have a tendency to fixate first on the left hand page and that retention is greater on the page receiving the primary attention. Whether the left side preference as indicated when observing a single page will still hold when viewing a double page (as in periodicals) will need to be determined by further experimentation.

Implications. In studying ocular patterns of different individuals by means of photography one is impressed with the tremendous importance of the eye, not only as a sensory but also as a motor organ. When we remember that the eye fixates from 100 to 225 times per min., and that an equal number of excursions are made in the same time, we begin to realize the service rendered by this organ.

The study of ocular patterns is significant, first, because it suggests that one type of performance is preferred over another; and secondly, it raises the question of whether this phenomenon is caused by the physiological structure of the sense organ and its accessory apparatus, its neurological organization, its habitual mode of behavior or a combination thereof.

If these findings have practical application, artists, educators, advertisers, and editorial writers may attach a relative attention-value to specified areas of the respective fields of observation. In that case, these principles of behavior should play a significant rôle in the efficiency of the individual in obtaining a maximum of information or satisfaction as he views the subject matter. If, for example, the first fixations fall at a point to the left and above the middle of the page, the advertising layout which utilizes this principle will have many advantages. First, the eye will not be distracted at the beginning of the survey; secondly, the direction for the course of eye movements can be initiated here; and thirdly, the mental content or

idea can have a right of way rather than to be forced by the physical stimuli of the presentation.

An adequate layout based upon this principle of observation would naturally organize all physical stimuli to facilitate the idea or purpose of the observer. The nucleus of the story would find a prominent place in the so-called optical center, in order to gain for the reader a proper orientation which would ultimately lead to a satisfactory completion.

If the eye prefers the horizontal to vertical movements, advertisements which require a preponderance of vertical movements will be read with much more difficulty or else will show only very poor coverage. Head or cutlines placed either at the top or bottom will rarely be seen. Left side preference as revealed by this study indicates that a more adequate use can be made of advertising space. If the common purpose of ocular movements is to gain for the individual such orientation as will be best for his present need, then each fixation, together with the excursion, implies inspection, selection, discrimination, and organization.

Since the eye movement technique analyzes more directly the process accompanying mental states, it may gain for teachers and supervisors a clearer understanding of the learning process, and furnish a basis for diagnostic and remedial procedures. Proper direction and instruction can only follow when the process itself is known. By means of ocular photography it should be possible to verify the assumption that achievement presupposes an efficient process.

THE EFFECT OF HYPNOSIS UPON VISUAL AND AUDITORY ACUITY

By KENNETH STERLING and JAMES G. MILLER, Harvard University

Though there is at present little disagreement that hypnotic suggestion can cause a decrease in sensory and muscular powers, and that it can cause an increase in maximal muscular performance, there is much disagreement as to whether sensory acuity can be increased under hypnosis. The only proper way to study this problem is by the use of control waking sessions similar in every important particular to the hypnotic session. In this paper is reported an attempt to increase by hypnotic suggestion visual perceptual acuity, visual sensory acuity, and auditory sensory acuity.

ANTECEDENTS

In the middle of the last century Braid[1] stated of hypnosis that: "In this process we have acquired the power of raising sensibility to the most extraordinary degree . . . and that any or all of the senses may be raised to the exalted state of sensibility referred to, almost *with the rapidity of thought.*" Henri Bergson in 1886[2] reported the case of a boy who, under hypnosis, read from the cornea of a man facing him the reflection of the 3-mm. letters of a book which the man was holding behind the boy's head. The whole image on the cornea could not have been more than 0.1 mm. high. He was also able to draw with the naked eye an object on a microscope slide 0.06 mm. high. Bergson did not ascertain whether the boy could have repeated these feats in the waking state.

In the same year Delboeuf performed experiments, cited by Bramwell,[3] on post-hypnotic recognition of time intervals. He used two young servant girls as *S*s, who were unable even under hypnosis to tell time well or to perform the arithmetic necessary to convert such large numbers of minutes as 1600 or 1150 into hours and minutes. *M*'s ability at arithmetic was better than *J*'s, and her results were more accurate. Delboeuf suggested under hypnosis that the girls would perform certain acts a specific number of minutes later. In 14 experiments, 6 were exactly correct, and 7 others were considered by Delboeuf as partially successful. No waking control sessions were used. Bramwell himself did 55 experiments of this sort with Miss D (also referred to as Miss A) in 1895–96.[4] *S* was told in a trance that at the end of a certain large number of minutes she should place a cross on a piece of paper and mark down what time she thought it was. Bramwell seems to assume that Miss D was unable to do the arithmetic involved

* Accepted for publication July 30, 1939. The authors express their appreciation of the assistance of Dr. Robert W. White of the Harvard Psychological Clinic in the development of their procedure.

[1] J. Braid, *Neurypnology* (*Braid on Hypnotism*), ed. A. E. Waite, 1899, 86 f.

[2] H. Bergson, De la stimulation inconsciente dans l'état d'hypnotisme, *Rev. philos.*, 22, 1886, 525-531.

[3] J. M. Bramwell, *Hypnotism*, 1913, 116-119.

[4] Bramwell, *op. cit.*, 119 ff.; also, Hypnotic and post-hypnotic appreciation of time, *Brain*, 23, 1900, 161-238.

for the reasons that (1) she often had difficulty with such problems, and (2) she was normally unable to remember such complicated numbers. Out of 55 attempts, only 9 were wrong, and they were only 2 to 5 minutes off. No waking control was performed. Like results were obtained with other *Ss*.

Moll, in the fourth edition of his book on hypnotism in 1909,[5] reported controlled experiments of his own that agreed with some work of Berger's showing that two-point discrimination is increased under hypnosis; he described like results for the pressure and temperature senses, but these were not controlled by comparison with the waking state; he said that D'Abundo produced enlargement of the field of vision under hypnosis; and he reported clinical results showing improvement in vision, smell, and muscular sense. Whether or not he always used controlled technique, he was aware of its necessity and explicitly critical of those who did not use it.

Bramwell, in 1913,[6] cited evidence to show that hypnotic suggestion can markedly improve vision, hearing, smell, lifted-weight discrimination, two-point limen, and temperature discrimination. All except two of these statements he based on clinical observation. In the experiment on lifted weights there was a control session in the waking state, but there is no evidence of great insistence on attending to the discrimination in the waking state, and there was an insufficient number of trials. The two-point limen experiment was not well controlled.

In 1914 Prince[7] made mention of an experiment in which objects were placed in the field of vision so far toward the periphery that they could not be seen clearly enough to be identified. Later they were accurately described in hypnosis. In another book Prince reported a similar experiment,[8] probably the same one, in which personality C of the dissociated patient BCA was the *S*. This may well mean that the results were abnormal, especially in the light of her behavior. The objects put into the periphery of vision were a match box, a pencil, a roll of paper (all of which *S* did not recognize except under hypnosis), a watch (which *S* recognized by its tick in the waking state), and a toy cat (which *S* recognized immediately with a shriek because of a former strongly emotion experience with a cat).

Travis in 1922 reported some experiments,[9] and Morgan discussed them at length in 1924,[10] in which a lowering or raising of the auditory limen was used as a measure of hypnotizability. Travis stated that in certain *Ss* a 'hypnagogic' state was produced by staring in reverie at a crystal. Those *Ss* who showed what was called a 'schizophrenic' tendency were negativistic, and their limens rose, while those who were suggestible and easily hypnotizable (called 'psychoneurotic') showed lowering of limen. These results were supposed by Travis to be due to a change not in sensory acuity, but in some central neural set. In this work the *Ss* were not hypnotized but 'hypnagogic,' but it was reported that[11] "those who

[5] A. Moll, *Hypnotism*, trans. 4th ed., 1909, 114 ff.
[6] Bramwell, *op. cit.*, 1913, 89 ff.
[7] M. Prince, *The Unconscious*, 1914, 57-58.
[8] Prince, *Clinical Experimental Studies in Personality*, ed. A. A. Roback, 1939, 346-347.
[9] L. E. Travis, Studies in dissociation, *J. Exper. Psychol.*, 5, 1922, 338-346; Suggestibility and negativism as measured by the auditory threshold during reverie, *J. Abnorm. Psychol.*, 18, 1924, 351-368.
[10] J. J. B. Morgan, The nature of suggestibility, *Psychol. Rev.*, 31, 1924, 463-477.
[11] Travis, *op. cit.*, 345.

obeyed the instruction and maintained the set to respond while they abstracted were enabled to react to a stimulus below their normal threshold."

In 1925 Young[12] reported that he could get no increase in tactual sensitivity under hypnosis (but, rather, a slight decrease), yet the *Ss* expressed a belief that they did better, which Young explained[13] as due to a "very great subjective intensification of the consciousness in regard to a touch which is actually felt." This work was controlled by sessions in the waking state. The same experimenter in 1926[14] performed an experiment in which one stamp was studied for 5 sec. through a half inch hole, which allowed to be visible only the face and part of the background. Then the stamp was shuffled with ten like stamps, and all were put under like holes. *S* was asked to pick out the stamp first seen. Six *Ss* did exactly as well waking as under hypnosis; for one other *S* the experiment was not completed.

In 1927 Lifschiftz reported great lowering of auditory limen under hypnosis in an experiment which was probably unrigorous.[15] Koster, in 1928, found that with 21 *Ss* he could by hypnosis bring about a diminution in muscular strength and discriminative ability in two thirds of the *Ss*, and also could obtain augmented muscular strength in two thirds, but could get increased discrimination ability in only one third.[16] In 1929 Fisher reported the case of a girl who was able under hypnosis to hear the ticking of a watch at fully twice the distance that she could in the waking state.[17] He says that many such cases of increased sensory ability under hypnosis have been observed. Of course in this case if *S* knew it was a watch ticking, she could under hypnosis have reported it even though she did not hear it, in order to please *E*.

Stalnaker and Richardson in 1930 reported a well-controlled experiment in which 9 *Ss* were tested for their ability to judge short intervals of time.[18] Altogether 1080 determinations were made, half waking, half under hypnosis. Each *S* had an equal number of trials for 1-, 2-, and 3-min. periods presented at random. None of the differences between waking and hypnotic judgments was significant, but the *Ss* thought they were doing better under hypnosis. Hull,[19] in 1933, referred to a minor experiment by Eken in which she found that the ability of persons to judge 16-min. intervals was not significantly different in hypnosis and in the waking state.

The literature reviewed is in conflict, but in the experiments which were apparently best controlled, and in the more recent work, the weight of evidence seems to indicate that hypnosis does not improve sensory acuity.

(A) Visual perception. The apparatus employed in the experiment in

[12] P. C. Young, Experimental study of mental and physical functions in normal and hypnotic states, this JOURNAL, 36, 1925, 214-232.
[13] Young, *op. cit.*, 222.
[14] Young, An experimental study of mental and physical functions in the normal and hypnotic states, this JOURNAL, 37, 1926, 345-356.
[15] S. Lifschiftz, *Žurnal nevropatologii i psichiatrii*, 20, 1927, 317-324.
[16] S. Koster, Untersuchungen über Hypnose, *Zsch. f. d. ges. Neur. u. Psychiat.*, 109, 1928, 49-61.
[17] V. E. Fisher, *An Introduction to Abnormal Psychology*, 1929, 432 f.
[18] J. M. Stalnaker and M. W. Richardson, Time estimation in the hypnotic trance, *J. Gen. Psychol.*, 4, 1930, 362-366.
[19] C. L. Hull, *Hypnosis and Suggestibility: An Experimental Approach*, 1933, 250.

visual perception was that used by one of the authors in another experiment and described in detail in a former article in this JOURNAL.[20]

Procedure. The *Ss* were led into a dimly lighted room and seated facing an 8-in. by 12-in. transparent mirror set in the opposite wall. *S*'s chair was about three yards from the mirror. During the experiment geometric figures were projected upon this mirror from behind at various intensities. The figures used were four of those on Rhine's ESP cards[21]—circle, square, plus, star. *E* made certain that *S* knew the figures and what they looked like when projected on the screen. They were almost the full size of the screen.

E sat behind *S* in a small room connected with *S*'s room through an open door. He sat behind a screen over which he could see *S*, facing a panel from which he could control remotely the exposure of the cards and the illumination at which the cards were projected. This illumination was regulated by a variac, which gave none at zero volts, and a comparatively dim image, though definitely supraliminal, at its top reading, 135 volts. By a periscope *E* could tell what figure was being presented on the screen, even at illuminations well below the limen.

Each *S* was tested during three periods: (1) a preliminary waking period, (2) a hypnotic period, and (3) a final waking control period.

After about 10 min. for dark adaptation, the first waking tests were made in the dark room. *S* was urged to concentrate as completely as he could. The figures were projected on the screen at random. The main purpose of these tests was to give *S* practice, to adapt him to the dark, and to determine the approximate region of his limen.

S was then hypnotized, and his state of hypnosis tested by obtaining eyelid and arm catalepsy. The dim room light was turned on for the hypnosis, but was turned off immediately afterward. After time for dark adaptation, it was suggested to *S* that he concentrate fixedly upon the screen, and that he had extraordinarily sharp and accurate vision. This latter suggestion was repeated continually throughout the hypnotic visual test. The four figures were presented at random in this period, each several times, *E* beginning at zero illumination and raising the brightness until *S* reported what the figure was.

S was awakened in the dimly lighted room, and then the light was turned off and he was given the same time for dark adaptation that he had been given immediately after hypnosis was induced. Then the average intensity at which he could just see each of the four figures presented at random was determined. The whole procedure lasted about 1½ hr. As has been noted in another article on an experiment in which this apparatus was used,[22] the limens for the various figures differ, but since the figures were presented at random in equal numbers, the effects of these liminal differences are not significant for the group as a whole. This has been determined statistically. The use of four figures at random eliminated the following difficulties: (1) confusion of after images with real images, and (2) preguessing the symbol, as might occur if only one or even two figures were employed.

[20] J. G. Miller, Discrimination without awareness, this JOURNAL, 52, 1939, 562-578.

[21] C. E. Stuart and J. G. Pratt, *A Handbook for Testing Extra-Sensory Perception*, 1937.

[22] J. G. Miller, *op. cit.*, 571-573.

Altogether in this visual experiment 9 *Ss* were used who developed eyelid and arm catalepsy, and whose data are included in this report. Those *Ss* from whom *E* could not obtain these symptoms of hypnosis were eliminated.

Results. The results are summarized in Table I. The CR of the paired differences between the averages for each *S* in hypnosis and in the waking state is 0.57. It will be noticed that the only *Ss* giving significant differences in the two states were negativistic, showing decreased acuity on suggestion of increased acuity.

All *Ss* accepted for these data gave the objective criteria of eyelid and arm catalepsy, and besides that *E* made the following judgments of degree of hypnosis: *S 1*, deep hypnosis; *2*, very light, awake by end of session; *3*, fairly deep; *4*, good light; *5*, good light; *6*, fairly deep; *7*, good light; *8*, very light; and *9*, deep.

TABLE I

VISUAL PERCEPTUAL ACUITY IN HYPNOSIS AND WAKING

Average illuminations (in volts) at which the geometrical figures were just perceptible.

S	HYPNOSIS					WAKING				
	plus	circle	square	star	av.	plus	circle	square	star	av.
1	57.0	62.5	58.3	—	59.3	66.5	61.3	65.3	66.7	64.9
2	74.3	78.2	76.7	110.7	85.0	67.5	62.5	65.3	98.0	73.3
3	50.3	50.3	55.6	64.7	55.2	40.3	47.4	46.0	62.0	48.9
4	70.0	69.5	68.5	72.0	70.0	71.5	67.0	72.5	80.0	72.7
5	67.0	61.2	59.3	85.0	68.1	53.3	54.0	53.2	72.5	58.3
6	95.0	64.6	63.9	114.0	84.4	80.7	51.2	57.8	85.0	68.7
7	95.8	82.7	88.2	71.7	84.6	81.5	86.3	87.5	84.0	84.8
8	61.8	58.4	65.8	79.3	66.2	55.0	60.5	80.0	83.5	69.8
9	62.8	64.4	67.4	81.8	69.2	—	58.0	59.3	70.5	62.6
Av.	74.4	65.8	69.3	84.9	71.3	77.0	61.0	65.2	78.0	66.0

(B) Visual sensation. This experiment deals with the effect of hypnosis upon discriminating the absence of a light from its presence, on the mirror used in Experiment A.

Procedure. Each session was preceded by a period of dark adaptation. The limen for each *S* was determined in the waking state by the method of limits, using 3 ascending and 3 descending series. *S* was then hypnotized and eyelid and arm catalepsy elicited. After suggestion that *S* now had especially acute vision, his limen was determined as before. Then *S* was tested again for arm catalepsy, which was obtained in each case. He was then awakened and his limen was determined again in the same manner.

Results. The results are summarized in Table II. It is apparent that the differences between the acuities in the two states were exceedingly slight.

Besides the eyelid and arm catalepsy criteria of depth of hypnosis, *E* made the following subjective judgments of the degree of the *Ss'* hypnosis: *S 1*, good deep; *2*, fairly light; *3*, good deep; *4*, good light; *5*, good light;

6, good light; *7*, good light; *8*, good deep; *9*, fairly deep; and *10*, good light.

(C) Auditory sensation. This work has to do with the recognition of the presence or absence of a single simple tone, rather than the perception of a more complex figure.

Procedure. *S* was taken into a dimly lighted soundproof room. He lay down on a couch, put on earphones, and covered the earphones and his ears with pillows. In his hands he had a signal key which, when pressed, flashed a signal light for *E*. The wires from the earphones led outside the room to an oscillator producing a pure tone of about 925~. On the output of the oscillator was an attenuator, which attenuated the constant voltage output from the oscillator from 0 to 100 db. by steps of 1.5 db. The tone was turned off by a shunt

TABLE II

VISUAL SENSORY ACUITY IN HYPNOSIS AND WAKING

Average illuminations (in volts) at which light was just noticeable

S	Preliminary waking period	Hypnotic period	Final waking period
1	33.8	36.0	34.8
2	37.0	36.5	36.0
3	33.1	34.3	34.3
4	35.8	35.8	34.1
5	34.3	34.5	34.3
6	37.1	35.8	35.1
7	33.7	33.8	33.8
8	32.5	32.3	32.0
9	31.1	31.3	31.3
10	39.8	39.1	38.7
Av.	34.8	34.9	34.4

across the output of the oscillator. The shunt was cut in and out by a silent mercury switch.

E instructed *S* to press the key during whatever period he heard a tone, and then left the soundproof room and shut *S* in. The method of limits was then used. *E* began at zero and added attenuation until *S* did not press the signal key for any of 10 short tones at that intensity. The intensity next above this was taken as the descending value. *E* then further increased the attenuation until it was well below the limen, and then raised the intensity of the tone in steps of 1.5 db., giving short tones at each step. When the light went on for one or more times, that was taken as the ascending value. For each *S* under these conditions, three ascending and three descending points were determined. The mean of these values was taken as the limen for this preliminary practice period.

S was next hypnotized and tested for eyelid and arm catalepsy. When these conditions were obtained, *E* suggested to *S* that he had extraordinarily acute auditory powers. The same procedure for obtaining the limen was repeated, three ascending and three descending values being found for each *S*. *S* was finally brought out of hypnosis, and the whole procedure repeated a third time, and a limen found for this final waking period.

Results. The results of the auditory experiment are summarized in Table III. Since the practice effect and adaptation were negligible in this experiment, the results from the preliminary practice session were con-

sidered sufficiently significant to be included. The CR of the first waking period with the hypnotic period was 0.66, and of the second waking period with the hypnotic was 0.11. In this experiment 70% of the trials lay within 1 db. of the limen value for that *S* in that period.

All the *S*s accepted for these data gave eyelid and arm catalepsy, and *E* also made the following subjective judgments of the degree of their hypnosis: *S 1*, light hypnosis; *2*, good light; *3*, good deep; *4*, light; *5*, light; *6*, good light; *7*, deep; *8*, fairly deep; *9*, light; *10*, deep, awake at end of session; *11*, deep, and *12*, very good light.

It is interesting to note, in corroboration of Young[23] and of Stalnaker

TABLE III

AUDITORY LIMENS (DECIBELS OF ATTENUATION) IN HYPNOSIS AND WAKING

S	Preliminary waking period	Hypnotic period	Final waking period
1	46.3	46.2	44.1
2	42.4	43.0	42.7
3	41.3	41.6	42.4
4	35.3	35.7	35.9
5	37.1	38.5	38.3
6	41.5	45.1	42.3
7	42.2	42.2	41.3
8	39.4	40.5	39.8
9	41.0	40.7	45.0
10	34.7	37.1	35.8
11	38.7	37.5	38.9
12	35.6	38.7	38.0
Av.	39.6	40.1	40.5

and Richardson,[24] that a majority of the *S*s in both visual and auditory experiments reported subjective certainty that they were doing better under hypnosis than in the waking state. As Hull has noted,[25] this may well have been the cause for some of the earlier reports of increased sensory ability under hypnosis.

CONCLUSION

The primary conclusion of these results is that it is not possible by hypnotic suggestion to lower significantly the limen for visual perception or sensation, or auditory sensation. There are at least three factors which must be considered in explanation of this result: motivation, attention, and sensory limitation. Was the limen not lowered because hypnotic suggestion does not increase motivation? Most probably not, because there is abundant evidence in the literature that hypnosis can increase motivation, as in the experiments which indicate that it can increase muscular

[23] P. C. Young, Experimental study of mental and physical functions in normal and hypnotic states, *loc. cit.*
[24] J. M. Stalnaker and M. W. Richardson, *loc. cit.*
[25] C. L. Hull, *op. cit.*, 248.

performances beyond the usual voluntary range.[26] Is it—in opposition to Fisher's hypothesis[27] that attention can be increased under hypnosis more than it can be by insistent requests in the waking state—because attention cannot be increased by hypnosis? That is probably one reason, for certainly there is so much evidence that attention affects the correctness of sensation and perception, that it must have been constant in both waking and hypnotic sessions in order to get the practically constant results of this study. Is it because it is impossible to increase the acuity of sensory end-organs beyond a physiological limit? Probably that is the most important factor. In the waking state, with practice and dark adaptation, the senses reached their full capacity. The attention of *S* was at a maximum, and even with motivation increased under hypnosis, he was physically unable to do any better. Unlike muscular work, it seems to be possible for sensation to reach its maximum efficiency in the waking state. The earlier uncontrolled experiments underestimated the possibility of reaching this maximum without hypnosis. This experiment indicates, therefore, as Travis[28] and others have suggested, that hypnosis has a central and not a peripheral effect. Hypnotic suggestion may hinder proper central organization of the response, but it cannot enable the individual to exceed the limits of acuity set by the peripheral receptor and the lower organizing centers.

Thus this experiment joins many others, such as Bass's,[29] which indicate that hypnosis is closely akin to the waking state.

What effect would hypnosis have had, if *S* had been required to guess about subliminal perceptions or sensations? It might well have increased the correctness of such guesses, but this result would have been due to the increased motivation under hypnosis, for increased motivation can counteract the boredom and feelings of frustration which develop in subliminal learning experiments.[30] Evidently correct guessing would, as in the waking state, be limited by the physiological maximum of acuity, and apparently hypnosis would effect little or no improvement in concentration over the waking state, although it might well keep attention from fluctuating for longer periods.

These results serve to add to the weight of evidence to the conclusion that hypnosis does not improve sensory acuity, and to extend the experimental findings in a quantitative manner to visual perception and sensation, and auditory sensation.

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[26] Hull, *ibid.*, 224-244. [27] Fisher, *loc. cit.* [28] Travis, *op. cit.*, 345.

[29] M. J. Bass, Differentiation of the hypnotic trance from normal sleep, *J. Exper. Psychol.*, 14, 1931, 382-399.

[30] J. G. Miller, The rôle of motivation in learning without awareness, this JOURNAL, 53, 1940, 229-239.

VARIABILITY OF JUDGMENT AS A FUNCTION OF STIMULUS-DENSITY

By JOHN VOLKMANN, Columbia University, WILLIAM A. HUNT, Wheaton College, and MILDRED MCGOURTY, University of Iowa

In studying affective judgments, Hunt and Flannery have shown that affective variability, defined in terms of the number of changes in judgment, increases with the number of stimuli to be judged.[1] In a previous article Hunt and Volkmann had shown that the anchoring effects demonstrable in judgments of visual inclination can be duplicated with affective judgments.[2] They suggest the operation of general principles of judgment, common to both affectivity and psychophysics. If some common principles do indeed apply, it should be possible to duplicate the results of Hunt and Flannery on variability in the affective judgment with conventional psychophysical material, as, for example, with lifted weights. While the present experiment is designed to test this possibility, it has certain other purposes as well.

In the first experiments of Hunt and Flannery the addition of more color-stimuli may have had either or both of two effects: an increase of the total affective range covered by the stimuli, or an increased density of stimulation within a fixed affective range. The range is defined as the difference in affective value between the most pleasant color and the least pleasant color; the density is defined as the number of affective stimuli per unit of range. It is probable that the addition of more color-stimuli increased both the affective range and the density of stimulation and, in advance of experimental information, it remains possible that variability of judgment is a function of both affective range and stimulus-density. Stimulus-density was probably, however, the major independent variable, since Hunt and Flannery attempted to keep range approximately constant by selecting stimuli which covered a wide range in each part of their experiment. In an effort to produce a parallel situation, the present psychophysical experiment holds constant the variable of stimulus-range and examines the effect of varying stimulus-density.

The stimuli were weights, arranged for lifting. In each part of the experiment the lightest weight was 45 gm. and the heaviest was 100 gm.; the stimulus-range was therefore held constant at 55 gm. The total number of stimuli varied from 3 to 12, and the intermediate stimuli were equally spaced between the values 45 gm.

* Accepted for publication June 12, 1939.

[1] W. A. Hunt and J. Flannery, Variability in the affective judgment, this JOURNAL, 51, 1938, 507-513. Note especially the trend of the results of individual *Ss* in Fig. 1 and Fig. 2, p. 509.

[2] W. A. Hunt and J. Volkmann, The anchoring of an affective scale, this JOURNAL, 49, 1937, 88-92.

and 100 gm. For example, the 3-stimulus set consisted of the weights 45 gm., 72 gm., 100 gm.; the 4-stimulus set consisted of the weights 45 gm., 63 gm., 82 gm., 100 gm.; and so on. Each stimulus-set was presented 100 times to each *S*, to yield 100 psychophysical series for record. The order of presentation was haphazard within single series. An arm-rest supported *S*'s arm and a screen cut off the apparatus from her vision. The stimuli were presented at a uniform rate of 1 in 10 sec. At a verbal signal given by *E*, *S* lifted the weight with a smooth wrist motion. The instructions to *S* called for absolute judgments in 3 categories. They were as follows:

I am going to present to you a series of weights which you are to judge. Make your judgments in the following way: call *1* for light weights, *2* for medium, and *3* for heavy. When I say 'ready,' pick up the weight under your hand and make your judgment.

Four women were used as *S*s, but the fourth did not complete the experiment. We have defined variability, following Hunt and Flannery, as a change in the

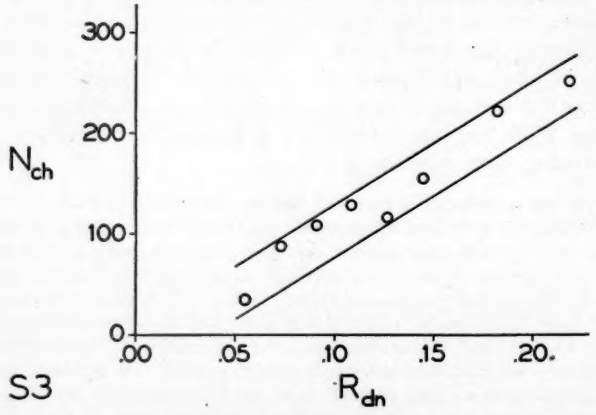

FIG. 1. THE NUMBER OF CHANGES IN JUDGEMENT AS A FUNCTION OF STIMULUS-DENSITY: DATA OF *S3*

category of report. This means that the amount of variability can be ascertained by counting the number of changes in the category of report for each weight during any experimental session. Thus, if a weight were called *heavy* (*3*) the first time it was presented, *light* (*1*) the next two times, and *heavy* (*3*) the last time, there would be 2 changes in the category used and the variability-score would be 2. The sum of the variability scores for all of the weights gives the total variability or number of changes (N_{ch}).

We are also presenting a second definition and measure of variability which is more nearly consistent with usual psychophysical practice. The variability represented by a psychometric function may be expressed as h (the index of precision in the Urban calculation), as a standard deviation, or as the semi-interquartile range,

Q. The last-named is the measure adopted here. Since the present experiment used three categories of judgment in each of its parts, it yielded two psychometric functions, just as the method of constant stimulus-differences yields two psychometric functions from its three categories *greater, equal,* and *less.* The Q's were actually determined by graphical interpolation from the data plotted on probability paper, which transforms the sigmoid psychometric data into approximately rectilinear form. The quartiles may be read off the plot without calculation. This process does not assume the normality of the underlying frequency-distribution. The Mean Q (Q_M) which appears in Table I and Fig. 2 is the mean of the Qs of the two psychometric functions.

TABLE I

SHOWING FOR THE NUMBER OF STIMULI IN EACH STIMULUS SET (N_R) AND THE CORRESPONDING STIMULUS-DENSITIES (R_{dn}), AND FOR EACH *S*, THE NUMBER OF CHANGES IN JUDGMENT (N_{ch}), THE MEAN SEMI-INTERQUARTILE RANGE (Q_M), THE WIDTH OF THE MIDDLE CATEGORY (W_2), AND THE CENTER OF THE SCALE OF JUDGMENT (C)

N_R	R_{dn}	*S1*				*S2*			
		N_{ch}	Q_M	W_2	C	N_{ch}	Q_M	W_2	C
3	.055	30	5.48	27.8	71.9	20	—	—	—
4	.073	72	4.30	26.8	70.5	81	6.08	26.7	76.6
5	.091	108	4.85	26.0	73.2	95	4.70	14.7	69.4
6	.109	142	5.23	27.9	74.7	94	4.03	12.1	57.9
7	.127	139	4.05	26.4	71.0	97	4.28	14.8	55.6
8	.145	188	4.30	29.2	68.7	98	3.78	13.2	55.7
10	.182	199	4.03	27.0	68.0	124	2.83	10.5	50.5
12	.218	342	5.45	26.3	70.2	93	2.45	10.2	50.9
N_R	R_{dn}	*S3*				*S4*			
		N_{ch}	Q_M	W_2	C	N_{ch}	Q_M	W_2	C
3	.055	34	—	—	—	77	—	—	—
4	.073	87	5.88	21.7	68.5	102	7.95	28.9	77.9
5	.091	108	4.60	25.2	69.1	116	8.15	31.5	85.2
6	.109	129	6.00	26.3	75.8	157	5.75	29.0	81.8
7	.127	116	4.80	24.9	71.3	204	5.90	20.1	67.7
8	.145	155	4.80	23.0	72.5	—	—	—	—
10	.182	222	4.73	22.1	72.3	—	—	—	—
12	.218	252	4.18	21.8	72.4	—	—	—	—

Table I shows for each of the 4 *S*s and 8 stimulus-densities the N_{ch} and the Q_M. Fig. 1 shows N_{ch} as a function of stimulus-density for *S3*. A straight line is a fair first approximation to the data of this *S* and the data of *S1* and *S4*. The data of *S2*, however, are markedly curvilinear, although the slope is still positive: N_{ch} increases with stimulus-density. Fig. 2 shows for *S3* the relation between Q_M and stimulus-density. Q_M is seen to be approximately constant; the slight decrease is probably an artifact of advancing practice. The order of experimentation began with low stimulus-densities and ended with high ones; a longer period of preliminary practice or a procedure which made the order of experimenta-

tion random might have avoided this slight decrease. The results of *S1* are very similar. The results of *S4* are too scanty to permit any generalization, and the results of *S2* are atypical since Q_M shows an abrupt decrease.

An inspection of Table I will show that W_2 is constant in the data of *S1* and *S3*. W_2 is a measure of the width of the middle category *2;* it is the central interval in the terminology of the method of single stimuli,[3] and corresponds to the interval of uncertainty in the method of constant stimulus-differences. The data of *S4* are again too scanty and the data of *S2* are again atypical, since they show W_2 decreasing instead of remaining constant.

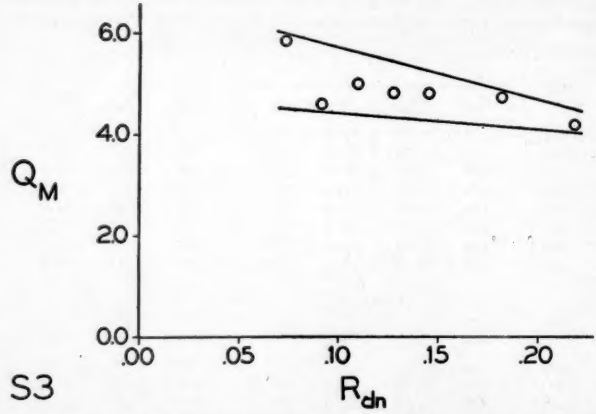

FIG. 2. THE MEAN SEMI-INTERQUARTILE RANGE AS A FUNCTION OF STIMULUS-DENSITY: DATA OF *S3*

The atypical results of *S2* deserve a separate treatment. Obviously some additional independent variable is at work; fortunately this variable is discoverable in the data themselves. As the order of experimentation progressed from low stimulus-densities to high ones, *S2* displaced her absolute scale more and more toward the lower end of the stimulus-range, so that the category *3* was used very often and categories *1* and *2* relatively seldom. This extreme displacement of the scale is shown in the decreasing values of C for this *S* in Table I. C is the center of the middle category, and *2* of the three-category scale. The other *S*s do not show

[3] The central interval is defined as the difference, taken without regard to sign, between the limen of the upper category and the limen of the lower category. Cf. J. Volkmann, The method of single stimuli, this JOURNAL, 44, 1932, 808 f.

any considerable displacements of C. Fig. 3 shows for *S2*, Q_M as a function of the proximity of C to the end of the stimulus-range. The measure of proximity is the difference, taken without regard to sign, between C and the end-stimulus which is nearer it, R_p, the proximal stimulus. As the scale is more and more displaced toward this proximal stimulus, the quantity $(\overline{C\text{-}R_p})$ decreases; and it will be seen from Fig. 3 that (following the data from right to left) Q_M decreases also. The apparent decrease of Q_M with stimulus-density is probably to be explained by this displacement of the scale. The decrease in W_2 mentioned above may be accounted for in a similar way, as a function of

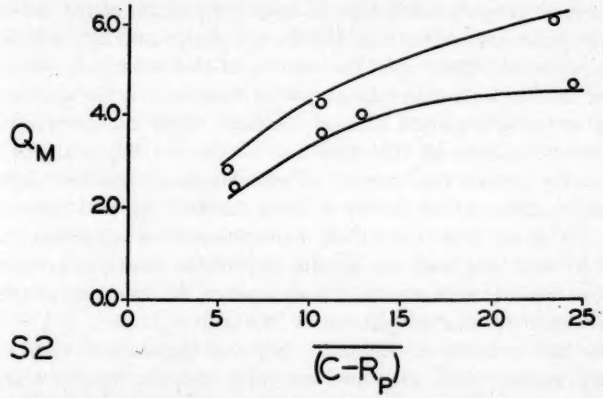

FIG. 3. THE MEAN SEMI-INTERQUARTILE RANGE AS A FUNCTION OF THE DISPLACEMENT OF THE SCALE OF JUDGMENT: DATA OF *S2*

displacement. Although this analysis of an exceptional case cannot establish a generalization, it does suggest one: displacement of an absolute scale toward either end of the stimulus-range produces a decreased variability and a decreased category-width. The attitudinal or other variables which originally produced the displacement are not known, so that the suggested generalization will be difficult to verify. As will be shown later, N_{ch} depends upon Q_M, so that if the variation of Q_M is atypical, as it was in the case of *S2*, the variation of N_{ch} would be expected to be atypical also.

Discussion. There is essential agreement with the finding of Hunt and Flannery that variability, as measured by the number of changes of judgment, increases as a function of stimulus-density. This agreement supports

the hypothesis that in the field of judgment there are regular relations which describe the judgment of any characteristic of any material. Other lines of evidence support this hypothesis: for example, the evidence upon anchoring in affectivity and psychophysics[4] and the evidence for the round-number tendency in the judgment of a great variety of materials.[5] There is increasing indication that judgment deserves the status of a separable field of investigation, since it appears to possess its own characteristic regularities.

The width of the middle category (W_2) remains constant as stimulus-density varies. Category-width is known to vary with stimulus-range;[6] the fact that category-width does not also vary with stimulus-density implies that category-width depends upon the position of the end-stimuli and not upon any intermediate stimuli. Q_M is also constant, and likewise seems to depend solely upon the position of the end-stimuli. The theory of the absolute scale must take account of these results; the absolute scale seems to resemble a linear chain of categories whose ends are constrained by the two end-stimuli. The constancy of Q_M and W_2 is all the more noteworthy because the variation of stimulus-density involves the simultaneous variation of an entirely different variable: the total presentation-time per series. Since the stimuli were presented at a uniform rate of 1 in 10 secs., and since an increase in stimulus-density necessitated the addition of intermediate stimuli in each series, the total time needed for the presentation of a complete series increased.

The two measures of variability, N_{ch} and Q_M, seem to yield contradictory results, since N_{ch} increases with stimulus-density while Q_M remains constant, but actually there is no contradiction. The precise relation between the two measures cannot be stated at the present time because the considerations involved in it are too complicated. It is not difficult, however, to gain an approximate idea of the relation. The essential feature is that Q_M is not dependent upon the number of stimuli within a given stimulus-range (the terms in which we have defined stimulus-density), while N_{ch} is.

The reliability of the determination of a psychometric function depends upon the number of stimuli which can actually be used in determining the function, just as the reliability of the determination of any function

[4] Hunt and Volkmann, *loc. cit.*

[5] J. E. Coover, Experiments in psychic research, etc., *Leland Stanford Junior Univ. Publ.*, Psychic Res. Monog., No. 1, 1917, 230-308.

[6] J. Volkmann, The compression of an absolute scale, *Psychol. Bull.*, 35, 1938, 676 (abstract).

depends in some degree upon the number of observed points. Each stimulus gives a percentage of judgments in a given category and in consequence one point which aids in the determination of the function of that category. Q is one of the constants of the psychometric function. *A priori,* then, the reliability of Q does depend upon the number of stimuli within the useful range, but the true mean value of Q does not. Empirically considered, *any* relation might obtain between Q and stimulus-density, if stimulus-density had an effect upon the discriminatory mechanisms. Apparently it does not have any effect (under the conditions of the present experiment), because Q remains substantially constant.

The case of N_{ch} is very different, for this is a measure which is indeed dependent on the number of stimuli. If the number of stimuli is increased and if the total number of series remains constant, the total number of judgments is of course increased; it might be argued that more possibilities of changes in judgment exist, and that therefore more changes occur by reason of chance factors alone. But this argument does not do justice to the actual situation. The distribution of judgments upon the stimulus-continuum is not random but regular; the regularity is expressed by the psychometric functions. Each stimulus yields a given proportion of judgments in different categories, and this proportion is one determinant of the number of changes in judgment. If an end-stimulus yields judgments which are all in one category, for example, there will be no possibility of changes in judgment on that stimulus. Intermediate stimuli, on the other hand, are likely to yield judgments in several categories and present many possibilities of changes in judgment. The number of changes in judgment which occur at a given stimulus value depends on at least the following variables: the number of categories, the proportions of judgments in different categories, and the intraserial effects. The determination of the intraserial effects is in its turn a complicated one. Some stimuli that are otherwise affected by the foregoing variables will produce more changes in judgment, and still others will produce fewer changes. Since the changes produced by the various stimuli are added to give the measure N_{ch}, the general effect is obviously to increase N_{ch} as more stimuli are inserted to increase the stimulus-density. The original contradiction is thereby resolved. It should be added that while Q is independent of N_{ch}, N_{ch} is not independent of Q. When Q varies there is necessarily a variation in the proportions of judgments in different categories, and a consequent variation in the possibility of changes in judgment.

N_{ch} is a useful measure of variability in those cases in which there is

no known relation between the psychological variable and a stimulus-continuum. This is often true, for example, in affectivity. It may be less useful just because it is not independent of the number of stimuli, and this limitation of the measure should be kept in mind. Only the use of a stimulus-correlation made it possible in the present experiment to determine empirically N_{ch} and Q, and the relation between them.

Summary

Stimulus-density is defined as the number of stimuli per unit of stimulus-range. As stimulus-density increases, the number of changes in judgment increases also. Originally found in affective judgments, this relation likewise is found in judgments of intensity of lifted weight; it may be, therefore, a relation which is general in the field of judgment. Category-width does not vary with stimulus-density, and variability of judgment does not vary substantially if variability is measured in terms of the Q of the psychometric function. There is no contradiction between the statement that the number of changes in judgment varies with stimulus-density, and the statement that Q does not.

RETENTION OF CONDITIONED RESPONSES TESTED BY EXPERIMENTAL EXTINCTION

By W. J. Brogden, University of Wisconsin

The retention of several types of conditioned responses has been reported for varying durations of time, with and without a decrement in magnitude of the response.

Hilgard and Marquis found retention of the conditioned eyelid reaction in the dog for periods as long as 3 mo. with some decrement in the amplitude of response.[1] Hilgard and Campbell report the retention of conditioned eyelid responses in man at approximately full amplitude for periods of time up to 20 wk., the longest interval tested.[2] Wendt found retention of conditioned flexion in a dog 2½ yr. after the original training, with a decrement in the magnitude of response.[3] Hilgard and Humphreys state that there was good retention of conditioned discrimination in man at all intervals tested, from 4–19 mo.[4] Razran reports that a lapse of 16 wk. resulted in nearly complete abolition of conditioned salivary responses in humans to single stimuli, and in their reduction by two-thirds to patterns of stimuli.[5]

The purpose of the present study was to investigate the retention at 6 mo. of greatly over-practiced conditioned flexion-responses in dogs by means of experimental extinction, where in some cases the response had been maintained by shock and in others had been maintained by food which had been substituted for shock.

Experimental Procedure

Within a soundproofed, light-shielded test-room, with *E* observing through a one-way glass screen, left forelimb-flexion to electric shock was conditioned to the sound of an electric doorbell. With four of the animals (1, 2, 7, 8) the

* Accepted for publication August 1, 1939. From the Pavlovian Laboratory, Phipps Psychiatric Clinic, Johns Hopkins University School of Medicine. This investigation was carried out while the author was a Fellow of the National Research Council in the Biological Sciences, 1936-37.

[1] E. R. Hilgard and D. G. Marquis, Acquisition, extinction, and retention of conditioned lid-responses to light in dogs, *J. Comp. Psychol.*, 19, 1935, 29-58.

[2] E. R. Hilgard and A. A. Campbell, The course of acquisition and retention of conditioned eyelid responses in man, *J. Exper. Psychol.*, 19, 1936, 227-247.

[3] G. R. Wendt, Two and one-half year retention of a conditioned response, *J. Gen. Psychol.*, 17, 1937, 178-180.

[4] E. R. Hilgard and L. G. Humphreys, The retention of conditioned discrimination in man, *ibid.*, 19, 1938, 111-125.

[5] G. H. S. Razran, Studies in configural conditioning: VI. Comparative extinction and forgetting of pattern and single-stimulus conditioning, *J. Exper. Psychol.*, 24, 1939, 432-438.

duration of the bell was 10 sec.; with two others (4, 5) it was 2 sec. When a perfect score of conditioned shock-avoidance was obtained (20 shock-avoiding flexion-responses to 20 presentations of bell), one biscuit of food was thereafter given at the end of each presentation of the bell to which Animals 1, 2, 4, and 5 responded with left forelimb-flexion. No food was given if flexion failed. The shock was omitted altogether. These animals were continued on the bell-food sequence for 1000 trials after the last shock reënforcement. The conditioned

TABLE I

EXTINCTION OF CONDITIONED FLEXION AFTER A LAPSE OF SIX MONTHS

F represents the percentage of conditioned flexion-responses evoked by the 20 representations of the bell given each test-period. L represents the mean latency of the conditioned responses tabulated in the next column to the left. S represents the percentage of conditioned salivary responses evoked by the 20 presentations of the bell given each test period.

Tests	Animals													
	1				2				4	5	7		8	
	F	L	S	L	F	L	S	L	F	F	F	L	F	L
1	90	2.11	10	2.00	60	4.83	5	3.00	75	20	90	2.50	95	2.23
2	95	2.57	15	7.33	70	4.00	40	3.33	65	0	100	1.52	95	2.29
3	100	3.45	5	3.40	20	4.75	0		5		100	1.89	100	3.21
4	100	2.35	0		0		0		0		65	2.63	90	2.16
5	60	3.00	0								100	1.52	100	1.78
6	90	3.88	0								95	1.21	100	2.80
7	40	6.12	0								100	2.28	100	1.85
8	5	9.00	0								100	0.99	100	2.33
9	5	8.00	5	4.40							100	2.72	100	1.89
10	0		0								75	1.83	100	2.08
11											100	1.58	100	2.38
12											50	2.92	80	2.08
13											85	2.55	95	2.16
14											95	2.58	50	2.28
15											80	3.01	80	1.88
16											95	2.26	55	1.74
17											45	2.57	30	2.20
18											75	3.93	55	1.78
19											45	2.20	60	1.87
20											50	4.18	45	1.80
21											35	3.43	0	
22											75	4.18		
23											0			

salivary response of Animals 1 and 2, thus set up, was recorded. Animals 7 and 8 were maintained on bell and shock for 1000 trials after their first perfect score. The results obtained under these conditions have been reported earlier in this journal.[6] When 6 mo. had lapsed after the last test, the bell was presented alone to each animal 20 times each day, until the flexion-response was completely extinguished (zero response to 20 presentations of the bell). The extinction of the salivary response of Animals 1 and 2 was recorded along with the extinction of the flexion-response.

[6] W. J. Brogden, Unconditioned stimulus-substitution in the conditioning process, this JOURNAL, 52, 1939, 46-55.

RESULTS

All animals showed a retention of conditioned flexion to bell at the end of 6 mo. (Table I). Retention was good, the frequency of conditioned flexion being above 60% for the first 40 trials for all except Animal 5. The character of the conditioned flexion-response had not changed, its magnitude, frequency, and latency being approximately the same on the retention tests as it was 6 mo. earlier. The conditioned salivary response of Animals 1 and 2 was poorly retained. It extinguished much more rapidly than did the conditioned flexion-response. This inferiority of the conditioned salivary response to the conditioned flexion-response is in keeping with the results obtained earlier. During the 1000 trials of bell-food, conditioned flexion occurred on the average 33.3% more often and 2.48 sec. earlier than did conditioned salivation.[7] The conditioned flexion-response of Animal 1 was completely extinguished in 200 trials, that of Animal 2 in 80 trials, of Animal 4 in 80 trials, Animal 5 in 40 trials, Animal 7 in 460 trials, and Animal 8 in 420 trials. The most striking thing about the extinction scores is the much greater number of trials required by Animals 7 and 8 to reach the criterion of extinction. The mean difference in the number of trials required for extinction between Animals 1 and 2 (M=140, S.E.=60), and Animals 7 and 8 (M=440, S.E.=20) is 300 (S.E.=63.25). This difference is statistically significant. The p value, revealed by the technique devised by Fisher,[8] is 0.01. Such a difference as this might be expected to occur by chance only once in one hundred times. Therefore, conditioned flexion originally reënforced with shock will persist without reënforcement after a lapse of 6 mo. over thrice as long as will conditioned flexion originally reënforced with food. Since conditioned flexion reënforced with food (occurring 977 times in the 1000 trials; mean of three animals) is functionally equivalent to conditioned flexion reënforced with shock (occurring 988 times in the 1000 trials; mean of three animals), the large difference in the number of trials required for extinction at the end of 6 mo. cannot be due to an inferiority in the level or amount of learning. The difference then must be due to the time-lapse or to a difference in the functioning of food and shock as incentives. If extinction immediately after the completion of 1000 trials of additional training revealed conditioned flexion reënforced with food to be equal in resistance to extinction to conditioned flexion reënforced with shock, the difference in extinction-time

[7] Brogden, *op. cit.*, this JOURNAL, 52, 1939, 46-55.
[8] R. A. Fisher, Application of 'Student's' distribution, *Metron*, 5, 1925, 90-104.

after a lapse of 6 mo. would be due to the effect of the period of inactivity. If on the other hand, immediate extinction revealed conditioned flexion reënforced with shock to require three times as long as conditioned flexion reënforced with food, the time-lapse would not be a factor in the present results. The difference in the extinction times of the two would then be a function of the action of the incentives, food and shock. Unfortunately, no such control data is available. Four animals, extinguished immediately after a score of 100% was attained in the bell-food situation, required a mean of 270 trials (S.E. = 149.40 trials) to reach the zero response-criterion. Six animals, extinguished immediately after attaining a score of 100% in the bell-shock situation, required a mean of 113 trials (S.E. = 29.60 trials) to reach the zero response-criterion.[9] The difference of 157 trials (S.E. = 152.20 trials) in favor of flexion reenforced with food is not significant; such a difference might be expected by chance eighteen out of one hundred times. While this data does not serve as an adequate control, since the animals did not have the overtraining of 1000 additional trials, it gives some weight to the view that the significantly more rapid extinction of conditioned flexion originally reënforced with food is a function of the lapse of time.

Summary

(1) Conditioned flexion responses in dogs showed good retention over a 6-mo. period. The magnitude, frequency, and latency of response did not differ markedly from the performance six months earlier.

(2) The conditioned salivary response of Animals 1 and 2 was poorly retained and rapidly extinguished, when compared with the good retention and relatively slower extinction of conditioned flexion in the same animals.

(3) The conditioned flexion-responses originally reënforced with shock required over three times as many trials for extinction as did the conditioned flexion responses originally reënforced with food. While no control data is available, the equality in the number of trials required in the immediate extinction of conditioned flexion responses reënforced with food and of conditioned flexion responses reënforced with shock points toward the lapse of time as the factor in the significantly longer extinction time of conditioned flexion reënforced with shock after 6 mo., rather than toward a difference in the functioning of food and shock as incentives.

[9] Unpublished data of the author.

APPARATUS

AN INSTRUMENT FOR DETERMINING VISUAL THRESHOLDS

By R. M. SIMPSON and G. L. FREEMAN, Northwestern University

Since the early investigations of Arago, many instruments have been devised for measuring visual thresholds. The visual "adaptometers" devised by Nagel,[1] Piper,[2] and Ferree and Rand,[3] as well as the "thresholdometer" of Luckiesch and Moss,[4] have been widely used. We shall not concern ourselves here with details of these several instruments, but describe instead one of our own construction and its particular advantages. The instrument (Fig. 1) consists in essential of a hollow tube, one end of which carries a weak stimulus-light and the opposite end an eyepiece through which *S* observes variations in the intensity of illumination of a ground-glass field. The instrument is used in a dark room and *S* or *E* may vary the intensity of the stimulus-light by changing the density of an intervening optical wedge.

As shown in the figure, the barrel of the instrument is mounted upon a heavy bar of channel iron. The regulation watchmaker's eyepiece (1) is mounted at one end of the barrel with a metal plate of 1.8 mm. aperture serving as an artificial pupil. The calibrated neutral-tint optical wedge,[5] attached to a chain belt running over sprockets and enclosed in the aluminum housing (2), is placed 12 cm. from the artificial pupil. The transmission of this wedge varies in a gradient from 1 to 100%. It is accompanied by another wedge with its gradient set in the opposite direction, thereby maintaining uniform density relationships in the system.

The sprocket-wheels used to move the chain to which the wedges are attached are rigidly fixed on shafts (3) near the ends of the housing. The wedges may be drawn in either direction by turning the geared friction drive adjustment (4) which is attached to one end of the shaft bearing the sprocket. The 4-inch wheel of this gear carries a graduated scale (5) which corresponds to the values of the optical wedge and is moved by vernier adjustment. This scale is calibrated in terms of the percentage of light transmitted as the wedge is drawn across the field of vision.

A 5.5 cm. aluminum barrel (7) extends from the side of the wedge-housing toward the stimulus end of the instrument. At a distance of 74.5 cm. from the artificial pupil a ground-glass fixation screen is inserted in a holder in the tube (8). A round hole 1.2 cm. is drilled through the center of the two walls of the screen-

[1] W. Nagel, Zwei apparate für die augenärztliche Funktionsprüfung: Adaptometer und kleines Spektralphotometer (Anomaloskop), *Zsch. f. Augenheilk*, 17, 1907, 116-118.

[2] H. Piper, Zur messenden Untersuchung und zur Theorie der Hell-Dunkeladaptation, *Klin. Monasbl. f. Augenheilk*, 44, 1907, 357.

[3] C. E. Ferree and G. Rand, A new type of instrument for testing the light and color sense, *Amer. J. Ophth.*, 14, 1931, 325-333.

[4] M. Luckiesch and F. K. Moss, A visual thresholdometer, *J. Opt. Soc. Amer.*, 24, 1935, 305-307.

[5] Eastman Kodak Company, No. JR-50.

holder to permit the light to pass through. The distance between the neutral-tint gradient wedge and the ground-glass observation screen is 62.5 cm. A 3.8 volt seasoned electric lamp (9) is mounted inside the barrel at a distance of 36.3 cm. from the ground-glass observation screen. The light is connected in circuit with a 1½ volt dry cell (10), a small ammeter (11) and a 6 ohm resistance rheostat (12). The ammeter serves to check the constancy of the current supplied to the light. The rheostat enables the experimenter to vary the intensity of the light as desired; use of the rheostat is essential in bringing stimulus-values of the threshold within the range of the optical wedge. An additional check upon the constancy of the

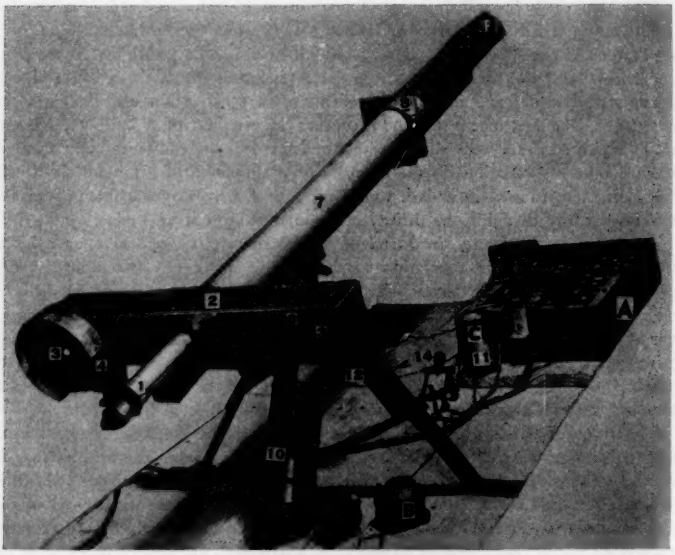

FIG. 1. THE NORTHWESTERN THRESHOLDOMETER

light source is a photo-electric cell (13) located at the stimulus-end of the tube, 8.5 cm. back of the light filament. The throw switch (14), shown in the figure, enables *E* to break the connection between the photo-electric cell and the ammeter and to use the latter instrument for measuring electrodermal responses (ABC).

Stimulus-values may be easily calculated from the instrument since the intensity of the light source with 100% transmission of the wedge is 0.000,000,034 lumens and the various settings of the wedge on percentages therefrom. Among the advantages of its use may be mentioned the fact that both *S* and *E* may make the adjustments for the minimum visible, that light intensity is carefully checked and measured at the source, and that visual thresholds may be determined by various psychophysical methods.

A DEVICE FOR MEASURING DIFFERENTIAL SENSITIVITY TO LIGHT

By Carl Fisk and S. Edmund Stoddard, Blackfoot, Idaho

For four years the authors have been experimenting with a new device, a photronic cell and a sensitive galvanometer, to measure an individual's sensitivity to changes in the intensity of light. The apparatus that we have constructed and describe here is inexpensive and easily made, hence it may readily be used in secondary schools as well as in colleges and universities.

A light-proof box was constructed 88 cm. long, 22 cm. wide, and 20 cm. high. Fig. 1 shows this box placed on its side with the lid removed. A partition, D,

Fig. 1. Apparatus Used in Measuring Differential Sensitivity to Light
Lamp B is stationary: lamp A is variable. The light falling on the photronic cell, P, is converted to electrical energy and measured by galvanometer, V, and then translated into foot-candles of light intensity.

divides the box into two longitudinal compartments. At one end of each compartment is a small circular aperture 2 cm. in diam. The distance between the centers of these openings is 5 cm. They are covered by a piece of ground glass, G, which has been removed in Fig. 1. The glass is held in place by wooden strips in such a manner that it can be withdrawn from one or both of the openings. The 7½ w. Mazda lamp, B, is stationary. The other 7½ w. lamp, A, is moved forward and backward by the rod, R. The light from the lamps is too intense, hence the lamps are covered by cylinders, C, in each of which is a round aperture 2 cm. in diam. The light from these openings falls upon the ground glass windows. The photronic cell, P, is fastened to a swivel enabling the operator to swing it easily over the window, lighted by the variable light source. As the cell is swung in place the ground glass is removed from the window.

The photronic cell, a Weston, is non-microphonic and comes to a maximum very quickly, declining in a few minutes to its final value of approximately 1% below maximum. Changes in temperature have only slight effect on current output though a relatively large effect upon the voltage that is generated. To measure this current output we selected a Leeds and Northrup reflecting galvanometer (V) that is sensitive to amperage changes but not to changes of voltage. Its sensitivity per millimeter at one meter is 0.00075 microampere. Its period is 13 sec. The external critical damping resistance is 25,000 ohms, and the coil resistance is 1132 ohms. The deflec-

tion of the galvanometer, when knife-switch N is closed, is read on scale Y by means of the eye-piece F.

The apparatus is operated as follows. (1) The photoelectric cell is placed over the window of compartment B and the illumination of lamp B (which is 7 cm. from the window) is measured on the galvanometer and recorded. (2) The photoelectric cell is then placed over the window of compartment A, and lamp A is adjusted to give the same galvanometric reading (*e.g.* 7 cm.). (3) Then the ground glass is placed over the windows and *O* is asked to judge their relative brightnesses which is (4) altered by moving lamp A nearer to or farther away from the window. (5) The liminal setting is obtained by some one of the psychophysical methods and that value

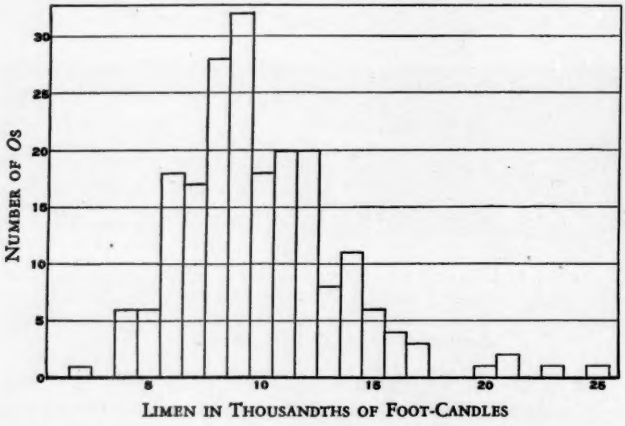

FIG. 2. BAR-GRAPH OF LIMENS OF 202 HIGH SCHOOL PUPILS

is translated into foot-candles by multiplying the setting in millimeters by the sensitivity of the galvanometer, and by dividing that product by the amount of current produced by the photoelectric cell. For example, a liminal setting of lamp A of 11.2 mm. was obtained for one *O*. This value is multiplied by 0.00075, the sensitivity in microamperes of the galvanometer one meter from the scale, and the product, 0.0084, is then divided by 1.4, the amount of current in microamperes that is produced when one foot-candle of light-intensity falls upon the photoelectric cell. The result, 0.006, is the limen in foot-candles of *O* one meter from the exposure-windows. In order that we may deal with whole numbers, this value is multiplied by 1000 and the limen expressed as 6.

The apparatus has been used with 202 pupils of the Blackfoot High School. The results are shown in Fig. 2. The lowest limen obtained among our *Os* was 2.4 and the highest was 25.55. The median value was 9.78, with the upper quartile at 8.1 and the lower quartile at 12.28.

NOTES AND DISCUSSIONS

Size Constancy and Emmert's Law

It does not seem to have been generally stated in the literature that Emmert's law implies size constancy when certain commonly accepted assumptions are made; and conversely.[1] There may, in fact, be some confusion about the relationship, for one law tells how size varies with altered distance, and the other how it remains constant with altered distance.[2] Perhaps the experts in the field of visual space perception are all sufficiently clear about this matter, although I have found two persons, both holding that the interdependence is obvious, disputing nevertheless as to its nature. I venture, therefore, to say how the relationship appears to me, fully aware that my choice of exposition is determined by my taste in logic, and that this matter can be expounded with equal right in other ways.

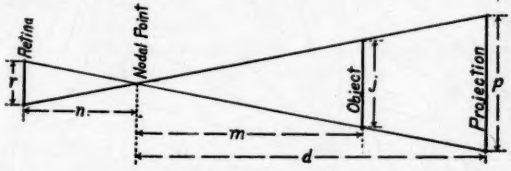

Fig. 1. The Optical Geometry of Emmert's Law

Fig. 1 represents the geometry of the physical optics that underlies Emmert's law. Its six quantities are defined as follows:

r = linear size of retinal image.
j = linear size of stimulus-object.
p = linear size of angular projection of stimulus-object on projection ground.
n = distance of retina from nodal point.
m = distance of stimulus-object from nodal point.
d = distance of projection ground from nodal point.

Now p is simply an objective size determined by the geometrical relationships of the figure, but let

$$s = \text{apparent size of } p.$$

Now Emmert's law becomes $s = k'p$

The usual form of Emmert's law is $s/d = j/m$, which expresses the geometry of Fig. 1, if $s = p$, as the law asserts. The constant of proportionality, k', reminds us merely that we are free to use any units of measurement.

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[1] On Emmert's law see E. Emmert, Grössenverhältnisse der Nachbilder, *Klin. Monatsbl. d. Augenheilk.*, 19, 1881, 443-450. It is odd that the law has received so little consideration when size constancy has received so much, but for some discussion see H. A. Carr, *An Introduction to Space Perception*, 1935, 365-368; K. Koffka, *Principles of Gestalt Psychology*, 1935, 211 f.

[2] Carr, *loc. cit.*, for instance, seems to me to be more impressed by the difference between the two hypotheses than by their mutuality.

Now it follows from Fig. 1 that

$$p/d = r/n$$

and thus by Emmert's law

$$s/k'd = r/n$$

So

$$s = (k'/n)rd$$
$$s = krd$$

The equation, $s = krd$, represents a hyperbolic surface and is the general law, since it implies both Emmert's law and the principle of size constancy. Because it is hard to visualize a surface, I have represented the surface by three sets of cross-sections in Figs. 2-4.

Fig. 2 shows how apparent size, s, varies with the distance of the field of projection, d, when the retinal size (or visual angle), r, is kept constant. Thus each line of Fig. 2 represents Emmert's law for some given retinal size; and the various

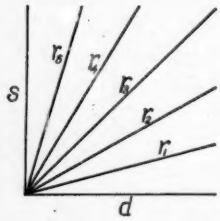

FIG. 2
EMMERT'S LAW
$s = k_1 d$, r = constant

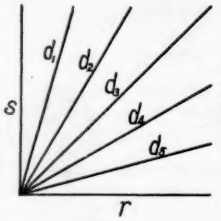

FIG. 3
DEFINITION OF APPARENT SIZE
$s = k_2 r$, d = constant

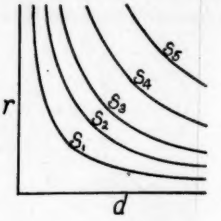

FIG. 4
SIZE CONSTANCY
$rd = k_3$, s = constant

lines of the figure are successive cross-sections at different r's of the surface $s = krd$. This figure does not by itself imply the general law, $s = krd$, because it states nothing about the law of variation of r.

Fig. 3 shows how apparent size, s, varies with retinal size, r, when the distance of the field, d, is constant. These relationships represent the operational definition of the magnitude of apparent size and are often overlooked, although essential to the argument. The apparent size of an object or image at various distances is measured by the physical size of a comparison object, which is kept always at a constant distance and which is equated in size to the test object or image. The lines of this figure represent successive cross-sections at different d's of the surface $s = krd$. This figure does not by itself imply the general law, $s = krd$, because it states nothing about the law of variation of d.

Taken together, however, Figs. 2 and 3 determine the surface, $s = krd$, and thus Fig. 4.

Fig. 4 shows a series of cross-sections of the surface, $s = krd$, taken at right angles to the cross-sections of Figs. 2 and 3. In other words, it shows how r varies with d if apparent size, s, is constant, and thus it is an expression of the principle of size constancy.

In general, it may be said that the three figures represent orthogonal sections of

the surface, $s = krd$. Any two of them determine the surface and thus imply the third. In the foregoing discussion the principle of size constancy was derived from Emmert's law by the assumption of a definition of apparent size. Conversely Emmert's law could be derived from the principle of size constancy by the same assumption; or it could be shown that, if both Emmert's law and size constancy are true, apparent size must be proportional to retinal size when distance is constant. The mathematical derivation of size constancy (Fig. 4) from Emmert's law (Fig. 2) and the definition of apparent size (Fig. 3) is as follows:

Let s_{11} = apparent size for d_1 and r_1, where d_1 and r_1 are any d and any r; and let s_{22} = apparent size for d_2 and r_2, where d_2 and r_2 are any other d and any other r. Start with s_1, d_1 and r_1, and change d_1 to d_2, but keep r_1 constant as a parameter.

$s_{12}/s_{11} = d_2/d_1$, since r_1, the parameter, is constant. This is the function of Fig. 2, Emmert's law. Hence

$s_{12} = (d_2/d_1)\, s_{11}$

Now keep d_2 constant as a parameter, and change r_1 to r_2. Then

$s_{22}/s_{12} = r_2/r_1$, since d_2, the parameter, is constant. This is the definition of apparent size, Fig. 3. Thus

$s_{12} = (r_2/r_1)\, s_{11} = (r_2 d_2/r_1 d_1)\, s_{11}$

$s_{22}/s_{11} = r_2 d_2/r_1 d_1$, which is the general law and may be rewritten with a constant of proportionality determining the units of apparent size:

$s = krd$

A special case of this law is

rd = constant, if s = constant, which is the function for size constancy in Fig. 4.

The other two derivations follow the same principle of treating the two variables successively as parameters, with $s = krd$ as the result. From that general law it is obvious that the three formulas of Figs. 2-4 follow at once.

The demonstration of the mutuality of Emmert's law and the principle of size constancy shows that the validation of the one hypothesis indicates the validation of the other, and conversely. It is not surprising, therefore, to find that it is both functions which fail under most conditions to be precisely correct as they are formulated here.[2] This note makes no pretense, however, of discussing the validity of either hypothesis; it seeks merely to show the nature of their relationship.

Harvard University EDWIN G. BORING

A National Analysis of the Psychological Articles Published in 1939

The writer has reported several times regarding the number of titles in different languages listed in the *Psychological Index* and the *Psychological Abstracts*.[1] A further break-down of similar results would seem to be of interest in two respects. (1) The increase in the number of titles listed under the caption "All

[2] On the departure of the size-distance function from size constancy and on various factors that determine the form of the function, see B. E. Holaday, Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *Arch. f. d. ges. Psychol.*, 88, 1933, 419-486; M. R. Sheehan, A study of individual consistency in phenomenal constancy. *Arch. Psychol.*, 31, 1938 (no. 222), 1-95. On the deviations of empirical fact from Emmert's law, see A. Noll, Versuche über Nachbilder, *Psychol. Forsch.*, 8, 1926, 3-27.

[1] S. W. Fernberger, On the number of articles of psychological interest published in the different languages, this JOURNAL, 28, 1917, 141-150; 37, 1926, 578-581; 48, 1936, 670-684.

others" (*i.e.* other than English, German, French, Italian, and Russian) has considerably increased during the last few years and one may be interested in the actual distribution of languages and the number of titles in this group. (2) There is interest in the *country of origin* of the titles listed as English to determine the relative number of British and American titles and of those written by authors in other countries.

The first question is answered by Table I. Complete results have been given for

TABLE I

BREAK-DOWN BY LANGUAGES OF TITLES APPEARING IN THE *Psychological Abstracts* IN 1939

	No.	%
Total in all languages	6557	
English	4229	64.5
German	1216	18.6
French	484	7.4
Italian	131	2.0
Russian	123	1.9
All others	374	5.7

	No.		No.
Japanese	150	Portuguese	11
Scandinavian	43	Greek	7
Roumanian	42	Czech	2
Spanish	41	Chinese	1
Polish	31	Estonian	1
Dutch	25	Finnish	1
Hungarian	19		

all language categories for articles listed in the 1939 volume of *Psychological Abstracts*. A total of 374 titles appear in languages other than English, German, French, Italian, and Russian. In this "All others"-group, at least 13 languages are represented and possibly more. The writer is not competent to differentiate between Swedish, Danish, and Norwegian and hence has classed all three of these languages as Scandinavian. It will be noted that there are a great number of titles in Japanese for this year—actually more titles in Japanese than either Italian or Russian for the same year.

Table II breaks down by country of the authors the English articles. Experience would indicate that such a break-down can be done, without a tremendous amount of labor, only by a psychologist who has been familiar with the psychological literature for many years. Only such an individual can recognize a large proportion of the names of authors and also know something of their origins and present whereabouts.

The method employed in the present study was to first examine the name of the author in the hope that he could be recognized and classified as to country. If the name was not recognized, search was made in the membership lists of the American and British Psychological Associations. If still unidentified, *Who's Who*, the *International Blue Book*, *Who's Who in Education* (American) and similar biographical references were consulted. If still unidentified, one sought to find the country of origin of the author by internal evidence in the title, the abstract or the publication in which the article appeared. It would seem that if an article appeared in a publication such as the Proceedings of the Indiana Academy of Science, that one may be reasonably sure that the work was done by an American. After all of these

criteria had been applied, there still remained 276 or 6.5% of the English titles which could not be identified as to the country of the author. Each of those few cases of multiple authorship by individuals of different nationality were assigned to the nationality of the senior author.

The results emphasize the volume of American work. Over 83% of all of the titles in English were done in America and certainly this number would be increased if we had been able to assign the unidentified cases. The number of British titles

TABLE II

BREAK-DOWN OF ENGLISH TITLES APPEARING IN THE *Psychological Abstracts* IN 1939

				No.	%
Total listed in English				4229	100.0
Unidentified				273	6.5
American (United States)				3525	83.1
British (Territorial, Dominions, etc.)				288	6.8
	No.		No.		
British Isles	255	Australia	9		
Canada	9	South Africa	4		
India	9	British in U.S.A.	2		
Other countries				143	3.4
	No.		No.		
Germans in U.S.A.	26	Poland	5		
Germany	25	Spain	4		
Scandinavia	25	Italy	4		
Holland	14	Hungary	3		
Russia	12	Belgium	1		
China	11	France	1		
Japan	11	Czechoslovakia	1		

is relatively small—actually smaller than the number of French titles for the same year even if the Colonial and Dominion titles are added to those of the British Isles proper. Of the British titles, all but 33 were written by authors living in the British Isles.

The largest number of English titles written by authors other than American and British is by the German group who have come to America within the last decade. In all, authors of at least 13 countries wrote articles in English which were listed in the *Abstracts* during the year 1939.

It is also interesting to note that the analysis shows that 54.6% of the titles listed for all languages have been identified as of American origin. If volume alone is considered, America is producing more titles in psychology and allied fields than all of the rest of the world put together.

University of Pennsylvania SAMUEL W. FERNBERGER

THE CORNELL SUMMER RESEARCH STATION IN PSYCHOLOGY

To meet the need, expressed by many psychologists, for a summer station at which study, research, and recreation may pleasantly and profitably be combined, the Department of Psychology of Cornell University established on trial last year the Cornell Summer Research Station in Psychology. Since the venture received encouraging support, it will be continued this coming summer for a second trial year. If the need for it is evidenced by attendance, the Research Station will be continued indefinitely.

Dr. Madison Bentley, emeritus professor of psychology and now psychological consultant on the staff of the Library of Congress, will be in attendance throughout

the session. He will assist the departmental staff in placing the facilities of the University at the disposal of the members of the Research Station and in arranging seminaries on subjects selected by them.

The psychological laboratories, seminaries, libraries, and the behavior farm will be open for research and study. The laboratories, comprising 66 rooms, occupy the two upper floors of Morrill Hall. Besides the usual equipment, they contain a large soundproof room shielded for work with bio-electrical potentials as well as for acoustical studies, a constant temperature room, a wing for small animal research, another for research in psychobiology, a seminary room, individual research and study rooms, and a machine shop with the services of a skilled mechanician. The University Library, adjacent to the psychological laboratories, is adequate for historical as well as for contemporary research. Admission to the library stacks will be arranged for those requesting that privilege. The behavior farm, where most of the animal work is done, is well equipped for work on conditioning.

Ithaca, situated on Lake Cayuga in the heart of the Finger Lakes Region of New York State, provides numerous facilities for summer recreation. Swimming, tennis, golf, horseback riding, boating, and sailing are readily available. Those wishing to enjoy less strenuous activities will find many interesting walks through the numerous glens and gorges. Four State Parks (Buttermilk Falls, Taughannock Falls, Enfield Glen, Watkins Glen), the show-places of the Finger Lakes Region, are all within easy reach.

Application for admission to the Research Station should be made to the Secretary, Department of Psychology, Morrill Hall, Cornell University, Ithaca, N.Y. Those holding the doctor's degree will not be charged tuition or any other fees. They will be given the status of Resident Doctor with all the rights and privileges of University membership. Others will be subject to the usual charges of the Cornell Summer Session.

Provisions have been made for three types of work: (1) rooms and apparatus for experimental research; (2) desk-space for library research; and (3) informal study. Since rooms and desk-space are limited, early application and attendance are suggested. Assignment will be made, within the limits of the provisions available for the various types of work, as applications are received.

The Research Station will be open from June 18 to September 1. Attendance may begin and end at any time within those limits. The Cornell Summer Session runs from July 8 to August 16. Since Resident Doctors are accorded the privilege of attending the special lectures, concerts, and excursions of the Summer Session, many may find it advantageous to come during those weeks.

K. M. D.

Postponement of the Twelfth International Congress of Psychology

An announcement has been received from Godfrey Thomson, the General Secretary of the Organizing Committee in Edinburgh, of the postponement of the Twelfth International Congress of Psychology which was planned to be held at Edinburgh, Scotland, in July of this year. The invitation of the City and the University of Edinburgh to meet there is not withdrawn. The members of the Committee hope that the Twelfth Congress may still meet in Edinburgh when circumstances permit the continuation and completion of the preparations.

K. M. D.

The Fourth Philadelphia Meeting of the Society of Experimental Psychologists

The thirty-sixth meeting of the Society of Experimental Psychologists was held on Monday and Tuesday, March 25 and 26, in Philadelphia at the University of Pennsylvania under the chairmanship of Professor S. W. Fernberger. It was attended by 27 members from 20 institutions, as follows: Brown (Graham, Hunter, Schlosberg); Cornell (Dallenbach, Weld); Duke (Muenzinger); Harvard (Beebe-Center, Boring); Iowa (McGeoch); Montreal Neurological Institute (Jasper); New York Psychiatric Institute (Landis); North Carolina (Dashiell); Pennsylvania (Fernberger); Princeton (Bray, Langfeld, Wever); Rochester (Culler); Rutgers (Pratt); Smith (Gibson); Stanford (Hilgard); Tufts (Carmichael); Vassar (Lanier); Virginia (Geldard); Washington (Nafe); Wesleyan (Wendt); Yale (Marquis, Miles).

Monday evening, at a banquet given to the Society by Chairman Fernberger at the Lenape Club, Professor Edwin B. Twitmyer gave an address of welcome and Dr. Ernest R. Hilgard was presented with the Howard Crosby Warren medal, the Society's award for outstanding research in experimental psychology, "for his analysis of the conditioned response and his demonstration of its integration with the verbal and volitional processes in learning and retention."

The 1941 meeting of the Society is to be held at Rutgers University under the chairmanship of Professor Carroll C. Pratt. The tentative dates are March 30 and 31.

K. M. D.

Leta Stetter Hollingworth: 1886–1939

Leta Stetter Hollingworth, Professor of Education in Teachers College, Columbia University, died on November 27, 1939, at the age of fifty-three years. She had served that institution continuously from 1916, receiving a professorship in 1930. To her colleagues in Education she will doubtless be remembered as an educator, but to her many friends and acquaintances in the field of psychology she was a psychologist. An examination of her researches and her numerous publications will clearly justify her title to a place among the eminent psychologists of her time. Even those facets of her work which were most clearly educational could rightfully be labelled clinical psychology. It is the psychologist rather than the educator to which these comments will refer.

A continuous thread of interest runs through all her scientific work, an interest in individual and group differences. An early association with the feminist movement led her to an objective attack in the laboratory upon some of the supposed sex differences held to justify the differing status of men and women in the world. Thus she studied sex differences due to functional periodicity in woman (1914) and sex differences in variability (1914). The latter problem she also attacked in an elaborate series of measurements of male and female infants at birth (1914).

Appointed in 1914 to the position of psychologist, newly created in the New York City Civil Service, she plunged actively into clinical work in hospitals, schools, and courts. Her research interest centered upon the characteristics of mental deficiency and of special mental disabilities. Her investigations and publications in these fields during the years 1914-1920 were systematically presented in *The Psy-*

chology of Subnormal Children (1920) and in *Special Talents and Defects* (1923). In the latter book there was foreshadowed her major interest of later years, the superior child.

Her critical insight in dealing with the problem children that came under her observation shortly disclosed to her the fact that poor adjustment is due not to defective intelligence alone but that emotions and attitudes play in it an important rôle. Their influence appeared to be particularly striking during the adolescent period, so that this age-group became the object of attack in her teaching and research. Courses on Mental Adjustments and Adolescence were given by her at Teachers College for many years. Ten articles on various aspects of adjustment were published, and the results of her years of study were presented in *The Psychology of Adolescence* in 1928. This is still a standard textbook in the field of mental adjustment.

The discovery of highly intelligent children among the maladjusted magnified an early interest in the superior child and led to the program of research which became her major life work. Although, out of her seventy-five published articles, some forty-five are devoted to the subject of the superior child, and one book, *Gifted Children* (1926), envisages the field as a whole, her work in it had really just begun. A survey of studies in progress and plans for the future gives some hint of the scientific tragedy of her death. To be sure, some of the material in manuscript can and will be prepared for publication, and some of the research under way will be completed, but the more ambitious parts of the program will scarcely survive the loss of her intelligent, enthusiastic and courageous direction.

A few of the uncompleted projects of particular scientific interest may be mentioned. One of these had to do with the development and future careers of gifted children. About 1920 Mrs. Hollingworth arranged with officers of the New York City school system for the segregation of a selected group of intellectually superior children less than ten years of age. They were intensively studied and their development followed over a period of twenty years and into adulthood. Such a project could never end as long as any of the original group survived. Even then their children and their grandchildren would be fruitful subjects for follow-up research.

One of her discoveries (1930), about which she was much concerned in her later work, was the fact that superior intelligence does not preclude serious maladjustments in the home, school, and later life but on the contrary carries with it peculiar hazards of its own.

An experimental school, established in February, 1936, by the New York City Board of Education and known as P.S. 500 or the Speyer School, with Dr. Leta Hollingworth as Director of Research, was a direct outgrowth of the earlier project, as well as a testimonial of its achievements. Plans were made for five years of research. The bright and dull child were both represented in this school, where unique methods and subject matters were put to the test and their effects measured. Many reports of the work of this school have already been published, and follow-up data should accumulate and be reported at intervals over many years.

Among the many studies in progress and incidental to this larger program, some educational and some psychological, were a sociometric study of gifted children in the school situation, a physiognomic study of the profiles and full-face photographs of gifted and slow-learning children, an investigation of the hands of gifted children, using objective measurements of dimensions and proportions, and a study of

the age at which demonstrable race attitudes begin to appear in white and negro children of various degrees of intelligence.

What might well have been the crowning scientific achievement of a life devoted to study of the exceptional child, when viewed in the light of generations of time and in the light of magnitude and extent of its influence upon society, was a plan for the establishment of a philanthropic corporation tentatively labelled "Superior Foundation." Its intended purpose was "to achieve a substantial endowment, to be directed intelligently toward the discovery, education and conservation as natural resources of the gifted young." What foundation could have a nobler purpose, when today the world bemoans the death of intellectual leadership and faces a future in which only intellectual leadership of the highest order can solve its problems?

This bare recital of scientific aims and achievements leaves untouched the record of a kindly sympathetic and at the same time vigorous and courageous personality. Her life began under frontier conditions in the middle west in an environment of sod houses, long-horned cattle and roving Indians. She had her introduction to education in a one-room log school house, a schooling which she later came to appraise as excellent in every respect. She entered the University of Nebraska at the age of sixteen, and graduated in 1906 with Phi Beta Kappa. She taught in a Nebraska high school for two years, resigning to be married to H. L. Hollingworth. From that time her life shifted to New York City where her husband was an assistant in the Department of Psychology at Columbia University.

She began graduate work at Teachers College and received the A.M. degree in 1913. Following this she held a part-time position in mental testing at the Post-Graduate Hospital and continued her work toward the Ph.D. at Teachers College. The degree was awarded in 1916.

Her scientific work on sex differences was accompanied by an active support of the feminist movement, which led her to march in the early suffrage parades and to serve as watcher at election polls. Her interest in the measurement of mental defectives was paralleled by participation in the active controversy as to who was properly fitted to give and interpret the tests. She vigorously defended the cause of clinical psychologists and was a leading spirit in building their professional organization and in raising the standards of mental examiners in universties and in the Civil Service. She did not hesitate to appear before school boards and state legislatures to plead her cause.

In her later years she became an ardent champion of the superior child and fought for his recognition and for a training program which would at the same time make the most of his intellect and enable him to develop a well-rounded personality. She gave most generously of her time and her personal funds for the support of research when institutions delayed or refused financial support. She followed the career of every one of her superior children, entertained them in her home and aided them financially when she felt such help was needed. Only her closest associates could know that no funds, though often sought, were ever received by her in support of her research.

While her personal sacrifices were heavy, she received her reward in the respect and affection of her superior children, in the esteem of her colleagues, and in the international recognition of her achievements.

Columbia University A. T. POFFENBERGER

Livingston Farrand: 1867–1939

Best known for his later contributions to public health and university administration, Livingston Farrand is remembered with appreciative affection by the older psychologists. For no less than nine consecutive years, beginning in 1896, he was Secretary and Treasurer of the American Psychological Association. During this period he was instructor and professor of psychology in Columbia University. For the last twenty-five years of his life he became an administrator, serving as President of Cornell University from 1921 to 1937. He died on November 8, 1939.

Born on June 14, 1867, graduating from Princeton in 1888, Farrand then attended the Columbia medical school. During his medical course he decided not to engage in the practice of medicine but to enter the field of education. His first step was to go over to the psychological laboratory, just established by Cattell, and to devote part of his senior year at the medical school to a special course in experimental psychology. The two following years he spent at Cambridge University and the University of Berlin, preparing himself for work in physiological psychology, and in 1893 he became instructor in this subject in Columbia University. He soon added anthropology to his academic offering and later came to concentrate his scientific interests entirely in the latter subject. He taught at Columbia for twenty years, the first ten in psychology, the last ten in anthropology. His courses in physiological psychology and ethnology were much appreciated by the early graduate students of psychology at Columbia.

He collaborated with Cattell (1894 *et seq.*) in setting up at Columbia what were known as the "freshman tests," a selection of anthropometric and psychological tests made each year on a sample of the entering college class, the purpose being to accumulate data for the study of abilities and personal traits, their development and correlation. This enterprise was carried on for many years and gave rise to some of the earliest correlational studies.

Meanwhile Dr. Farrand's ability as an administrator and organizer was becoming manifest and his services were enlisted, first for part time and then for full time, in public health enterprises and later in university administration. His personal charm, his quiet humor, his tolerance and understanding, his unostentatious courage, enabled him to bring men of divergent points of view together in harmonious cooperation, and to handle delicate and serious situations both in the War and before and after. The psychological fraternity is proud to have a share in honoring his memory.

Columbia University — R. S. WOODWORTH

Charles Augustus Strong: 1862–1940

Dr. Charles Augustus Strong was born at Haverhill, Massachusetts, on November 28, 1862. After studies at Rochester, Harvard, and Berlin, he taught at Cornell, Clark, and Chicago, and for a longer period (1896-1913) at Columbia. His courses at Columbia were on "Analytic Psychology" and the "Philosophy of Mind." His interest was in the philosophical aspects of psychology, as shown by the titles of his important books, *Why the Mind has a Body* (1903) and *The Origin of Consciousness* (1918). For many years prior to his death on January 23, 1940, Dr. Strong had resided near Florence, Italy, devoting himself to philosophy.

Columbia University — R. S. WOODWORTH

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

The Factorial Analysis of Human Ability. By GODFREY H. THOMSON. New York, Houghton Mifflin Co., 1939. Pp. xv, 326.

All who follow the development of factor theory or methods, or who apply the factorial techniques will surely wish to read this volume. Its author is well known as an early critic of Spearman's theory and as the originator of a rival conception of mental ability known as the sampling theory. In the course of years he has also been a contributor to the underlying mathematical rationale and to the development of computational devices in the field of factor analysis. In presenting an organized exposition of extant theories and methods, the author brings to bear his own experiences and contributions.

Because computational procedures in factor analysis have been reduced to routine steps, many investigators have been enticed to employ those procedures, too often without the restraints imposed by a more thorough knowledge of the underlying mathematical theory and of its applicability. Fortunately, Thomson gives very clear accounts which can be grasped without the aid of greater mathematical sophistication than is afforded by elementary algebra, geometry, and trigonometry. A very brief mathematical appendix is provided for those who are on terms with advanced algebra.

Early and frequently the author pays deep respects to Spearman, and like most so his countrymen, holds the conviction that a *g* factor will always be found among tests of ability, no matter whatever else may or may not be found. Says Thomson, "The main idea which still, rightly or wrongly, dominates factorial analysis was enunciated by him, and practically all that has been done since has been either inspired or provoked by his writings" (p. 5). As forecasted in his earlier writings, Thomson harmonizes his own theory with the *g* hypothesis.

In early chapters the theoretical bases for the two-factor, sampling, and multiple-factor theories are laid. In every case, the assumption is made that any correlation between two tests is due to a great many small, identical elements, a conception of correlation which is at least as old as Poisson but which few writers accept explicitly. For Thomson, the psychological elements, or 'bonds' are the real constituents of mental ability and this conception dominates his theory. All other views are to be reconciled if we admit that the factors are "only statistical coefficients, possibly without any more 'reality' than an average, or an index of the cost of living, or a standard deviation, or a correlation coefficient—though on the other hand, it may be that some of them, Spearman's *g* for example, may come to have a very real existence in the sense of being both useful and influential in the lives of men" (p. 42).

His main support for the unreality of factors is given in his discussion of the effects of sampling. The factors and their loadings, he maintains, have no absolute meaning but are functions of the selected population of individuals and of tests. With this result few factorists would probably disagree. Different factors and different loadings do arise from different samplings of individuals, and in some methods, from different samplings of tests. But the conclusion is not inevitable that factors are therefore not 'real.' His elemental 'bonds' of the mind, however, he

maintains are real, and are associated somehow with the "one hundred thousand million (sic) nerve cells" in each normal brain. On the basis of these atoms of ability one should expect Spearman's criterion for a single common factor to hold, particularly when a sufficiently large number of bonds are brought into play in tests. There is apparently no retreat from Thomson's early position that "the laws of probability alone will account for a tendency to zero tetrad differences among correlation coefficients" (p. 47).

The geometry of mental abilities is presented in describing the various theories. Procedures for computing factor loadings by the methods of Spearman, Thurstone, Hotelling, and Burt are described. Holzinger's work receives but slight mention. Thomson points out that Spearman's and Thurstone's methods maximize the importance of specific factors in their attempt to find as few common factors as possible, whereas the method of principal components minimizes the specifics in favor of the common factors. He warns that a really accurate solution of any factor problem is impossible because, considering all the specifics and error factors, there are always more unknowns than there are equations. He believes that Thurstone's centroid method, assuming as it does the criterion of simple structure, will fail to find a *g* factor and that the admission of oblique axes defeats the desire of the factorist to substitute a few uncorrelated variables for the original correlated test variables. He admits the validity of Stephenson's inverted analysis, in which persons are correlated instead of tests, but does not agree that the factors thus obtained will coincide with the usual ones unless we start with covariances instead of correlation coefficients.

The book is full of information about factor analysis, and in spite of rapid developments in this field will serve as a standard reference book on the subject for some time.

University of Nebraska J. P. GUILFORD

The Logic of Modern Psychology. By CARROLL C. PRATT. New York, Macmillan Co., 1939. Pp. xvi, 185.

Fundamental problems of psychology are examined thoroughly and critically in this volume. Anyone who has made a serious attempt to clarify his own concepts of the scope and methods of psycholgy will receive Pratt's considerations with interest.

In a lucid and often humorous style, some of the most troublesome psychological questions are reviewed: (1) What is the subject matter of psychology? (2) What is the relation of the various schools? (3) Can scientific psychology be coordinated with the layman's common sense observations? (4) How can satisfactory definitions of psychological concepts be arrived at? (5) In what relation does psychology stand to physiology? (6) What are the limitations and possibilities of hypothesis? (7) What are the boundaries of psychology as science? (8) To what extent may psychology be applied to practical problems?

The introduction states briefly the author's answers to these questions as the theses upon which the body of the book elaborates. Had one chapter been devoted to each problem the organization of the exposition might have been improved; as it is, however, the answers are all present.

(1) The subject matter of all sciences is the same. There is no identifying mark on the observational data of psychology which sets it apart as a study of 'mind,'

a *bete noire* which is competently disposed of. (2) Much less is there any real division between the systems which have arisen within psychology itself. A brief but pointed consideration of 'schools' since Wundt successfully maintains this position.

(3) Layman and philosopher are taken for a rough ride over fallacies in their confused concepts of self, intuition, and meaning. The author concludes that what the psychologist does with introspective data, his "manipulation of data by the instruments of logic," does indeed justify his recognition as a scientist and differentiates his work from the subjective considerations which go to make up literature or philosophy. Science is differentiated from casual observation by its careful description of the methods which produced the data and by the fact that both methods and data are public knowledge. "If scientific knowledge could be regarded as identical with the mere having of experience the task of science would be simple indeed. Every man would be his own scientist, and all of his experiences would immediately become true items in the general body of scientific information." Experiences, however, can be made public only by the use of symbols, words, and it is in communication that many all-too-familiar difficulties arise. (4) Pratt's consideration of these symbolic words, concepts, and their misunderstanding is pertinent, his analysis of discrepancies between facts and concepts is clear, his suggestion for avoiding the pitfalls of loose word-meanings follows Bridgman's scheme of operationalism, *i.e.* the equation of word-meanings to the operations and observations which led to the use of the words. Considerable space is devoted to the subject of meanings and definitions and comprises one of the most interesting and valuable sections of the book.

(5) Objections which have been raised against the tendency of psychology to move in the direction of physiology are stated and refuted as Pratt moves to the conclusion that "psychology is in a position to construct its own formal physiology without waiting for material physiology to catch up with it." His argument is well supported and convincing. (6) In elaborating this argument he clearly indicates the importance of the hypothesis in science and exemplifies the construction of logical theory on the basis of observed facts in Sherrington's description of the synapse.

(7) In defining scientific knowledge as the understanding of "conditions which underlie a thing and the quantitative influence which these conditions exert" the author simplifies the task of delineating psychology from common sense and from some disciplines which have been confused with scientific psychology. Freud, for example, is recognized as a literary psychologist, not a man of science. The value of statistical surveys and the case history method is, quite legitimately, questioned. This discussion then leads easily into the answer to the last question, (8) What applications of psychology are possible? Having pointed out that "the important determinants of man's higher activities are inside of him . . . (hence) to apply the method of concomitant variations to them is still out of the question" it follows that interested onlookers shall be disappointed when they seek aid from the psychologist in solving problems of social adjustment, leadership, education and the like. "Treatment of maladjustments," Pratt observes, "is more of an art than a science." It is unfair and dishonest for the psychologist to make great claims for the efficacy of his science in these lines. While admitting that the ultimate justification for psychology is in its practical contributions he emphasizes that everything is to be gained by sticking to pure science for the nonce. He voices the opinion of many a

psychologist in saying that "if the general public is not fully aware of what is going on there (in the library and the laboratory), so much the better."

While Pratt's theses may have been shared by others for some time, his well thought out discussion is useful for its survey of vital problems and for crystallizing points of view. Though the internal organization of the book is not always as clear as it might have been (chapter headings and subheadings could be changed to lead the reader more easily from one main question into another) the book is on the whole coherent, stimulating, and well worth reading.

Colorado College MARTHA LOU LEMMON

The Failing Student: A Study of Academic Failure and the Implication for Education. By KENNETH L. HEATON and VIVIAN WEEDON. Chicago, University of Chicago Press, 1939. Pp. x, 286.

This study was made "under the supervision of the Coöperative Bureau for Educational Research as an aid to institutional improvement in Michigan colleges" (p. v). It concerns itself chiefly with the failing group of students in four Michigan colleges: Albion, Central State Teachers, Michigan State, and Olivet. The group selected numbered 938 persons, or about 12% of the total enrollment in the four institutions.

It was found that failing students do both good and poor work in every branch of study. As judged by the American Council aptitude test, one-third of the failing group ranked above the average. As to reading ability, though 39% ranked in the lowest quartile, these students did not necessarily have low scores in the aptitude test. Individual examination revealed many instances of imperfect study habits and skills, from which the conclusion is drawn that the college should identify individual differences at entrance and thereafter individualize the instruction.

As regards finances, vocational motivation, physical defects and general health, personal and social factors which might impair study, there appeared no notable difference between the failing group and successful students.

Women students did not rank much higher than men in the aptitude test, but the proportion of men in the failing group was much larger than that of women. From this fact the authors conclude "that men students are facing a problem of adjustment which is apparently more difficult than that faced by young women" (p. 207). No reason is given for this difference.

The most illuminating chapter deals with "College Experiences as Appraised by Students," in which reports were obtained not only from the failing group but from an equal number of successful students. From these appraisals we learn, among other things, that "many required courses seem unimportant to students" (p. 211). "Student interests are followed in planning some courses and neglected in others" (p. 213). "Students are essentially non-critical of materials and subject-matter, and seem rarely to know of materials other than those provided or recommended by the instructor" (p. 214). "There is very little evidence that students have thought of college courses in relationship to permanent interests" (p. 217). "Courses that seem of little or no value are likely to be neglected, while those that seem important are easier ones on which to work" (p. 218). "A majority of students feel that there is a lack of variety in teaching methods" (p. 221). "Most students think that discussion should be an important part of college courses" (p. 224). "Few students are

familiar with what is commonly called 'self-appraisal' " (p. 229). "Dishonesty in the securing of grades is considered common" (p. 229). "Students turn more often to fellow-students than to faculty members for guidance" (p. 240).

Perhaps no clearer evidence could be had of the failure of the college to educate its students than is recorded in these comments. For those who maintain "that the college exists for intellectually superior students . . . it is disturbing that so many potentially superior individuals are falling behind in the struggle for an education" (p. 245). The authors believe "that the college does not exist solely for the superior student but that it has a contribution to make to the general education of a large body of the total population . . . and that it has at least a limited responsibility for all who may care to continue study beyond high school and also may wish, thereby, to push back the personal and social horizons of life" (p. 246).

Although the authors are hopeful that improved methods of guidance may achieve these ends, we learn little from this inquiry which is calculated to warrant such hopefulness. It does not seem to have occurred to the writers that without a genuine interest in scholarship it is impossible to pursue a higher education with profit. No matter how much we know or may be able to learn of individual differences, no matter how much we may individualize study with regard for these differences, unless the individual should awaken to his own need for knowledge and mastery of his subjects he will continue as he now does to 'get by' with a minimum of effort and understanding. Without insight one gains no understanding of anything, and without a command of linguistic, mathematical, and other tools of expression, one gains no insight which will "push back the personal and social horizons of life."

The implication of this study for education, though it is nowhere precisely stated, is that "though you may send a boy to college you cannot make him think." A further implication is that in our effort to educate everybody we are perilously near the point of educating nobody. If the failing group of students includes many persons who are potentially as able as those who succeed, the problem that confronts us cannot be solved by guidance, but only by discipline. In a process of mass-education which neglects the exacting discipline of a strict training in linguistic, mathematical, and graphic means of expression no one can be properly educated. Only with the aid of 'tools' of knowledge like reading, writing and speech, calculation, the graphic arts and the ability to construct things, is insight afforded which will provoke thought and lead onwards to the competency of the educated person.

So long as your young people are sent to school to mark time until they are old enough to find jobs in the world, the motivation essential to an education will be weak. Unless we can devise means for making the acquisition of the 'tools' of knowledge strict and disciplinary we shall continue to ruin the prospects both of those able and of those unable to profit by the well attested subjects of higher education.

Cornell University R. M. OGDEN

Mental Tests, Their History, Principles and Applications. By F. N. Freeman. Revised edition. New York, Houghton Mifflin Co., 1939. Pp. x, 460.

The reviewer, being no expert in psychometrics but, like many another, confronted with the problem of choosing a textbook suitable for a college class in measurement of intelligence and personality, proposes to evaluate the present book from that

angle. This means that he will not judge it as an advanced or exhaustive treatise, which it is not, nor as a manual to be used by mental testers, which few students become. In fact, no book that demands a technical knowledge of statistics would do. He will, however, expect it to be as up to date as possible in this rapidly developing field, and to give an accurate and comprehensive survey of mental tests as they are currently used. He will expect some evaluation of the tests discussed, and some guidance in their critical use, in view of the tremendous range of difference in the validity of these various measuring instruments, and in the significance attached to them by expert opinion. Finally, he will look for an adequate theoretical discussion of the nature of intelligence and personality to give the background for interpreting the whole testing movement.

To what extent has the present text met these requirements? Knowing the author, one would expect a conservative, cautious and unbiased treatment. This expectation is, with some exceptions, borne out, especially so in Chapters I, XV, and XVI, which deal, respectively with the present status, meaning, and fields of application of mental tests; the interpretation of mental tests; and the nature of ability. No misguided layman, prejudiced either by an over- or under-confidence in psychological tests, could possibly read the sane appraisal contained in these chapters without profiting greatly by it. Both the limitations and advantages of the method are plainly presented. A good bibliography of texts in the field is appended to Chapter I.

Students often puzzle over Spearman's tetrad difference method of proving his two-factor theory of ability. This and other uses of correlation are presented in such a simple, elementary way in Chapter III as to be clear to anyone. The same can be said of Chapters X, XI, and XII, which discuss "the technique and theory of mental tests," meaning such problems as the selection of materials and organization of items, and the setting up of norms, and comparison of various types of scores. The handy table of equivalent measures of brightness on page 302 is credited to F. A. Kingsbury.

The discussion of the various types of intelligence test proceeds historically and developmentally, with clear differentiation of age and point scales, language and performance tests, etc. The chapter on "Tests for the Analysis of Mental Capacity" deals with special abilities tests, like those for mechanical, musical, and art talent. The instructor would need to supplement the brief descriptions with a much fuller analysis to make this part of the book effective.

Chapter VIII, dealing with tests of personality traits, is hardly adequate. Here, if anywhere, is needed a highly critical evaluation. The discussions of tests like the Vineland Social Maturity Test, and the Willoughby Emotional Maturity Test, are too sketchy, considering that these are prototypes along the lines of which further tests of these traits will in all likelihood develop. Granted the excuse that they are too new to be properly sized up, how about other earlier experimental attempts like the Downey Will Temperament Test? We find only casual mention of the fact that the consensus of opinion has relegated it to the discard; yet its validity is defended purely on the basis of the findings in the author's identical twin study in which four out of nineteen profiles came out nearly identical. What does this prove? One also looks in vain for a discussion of the logic of types and traits, which is practically indispensable to an understanding of the reason for the rejection by experts of the whole type theory and of the tests designed to isolate types. A good

selected bibliography of tests of personality is appended. If supplemented by collateral readings, the book is quite usable.

University of Cincinnati A. G. BILLS

Psychology for Business and Industry. By HERBERT MOORE. New York, McGraw-Hill Book Co., 1939. Pp. xi, 527.

This book purports to cover the field of business and industrial psychology. Actually, of the fifteen chapters, thirteen deal with personnel (employment, testing, training, promotion, industrial relations), and the remaining two with advertising and selling.

Moore states that his purpose is to bridge the gap between laboratory findings and practices which have been found to be efficacious in business. Before the reader arches his eyebrows at that heresy, let him think of this from a standpoint of validity rather than of reliability and laboratory controls. Too much has been written by theorists who are unaware of practical limitations. A little experience in selling, advertising, or doing personnel work would have changed much that has been written.

The author made an excellent choice in opening his discussion of personnel procedures with figures on the extent and cost of turnover. Often this is one of the concluding points, yet nothing will so impress one with the necessity of proper selection as to know what a mistake will cost.

It is possible that too much space is devoted to tests, particularly since the author gives such an excellent critical evaluation of their uses, misuses, and shortcomings. Chapter VI has twenty-six pages on "Introducing and Constructing Tests." This will probably not be of much interest to the majority of readers, whether students or persons in industry, as not many have occasion to devise new tests. The same criticism might be directed to the lengthy discussion of rating scales in Chapter VIII.

Two whole chapters and parts of several others are devoted to employee relations. The 'jazz era' of the 20's was the testing decade in personnel. If the reviewer reads the signs rightly, it seems as if the trend in the 30's has been toward dealing with the worker after he has been employed. Chapter XIII, "The Problem Employee," is novel, interesting, and valuable. Not only ethically are mental hygiene services preferable over cold-blooded dismissal, but from a selfish viewpoint a concern will save by straightening out an emotionally maladjusted worker than consuming time and money breaking in his successor.

The chapter on accidents goes into the causes, or attending circumstances, very thoroughly. It is perhaps unfortunate that more space was not devoted to prevention. This shortcoming is characteristic, however, not only of this book, but of nearly all in this field.

The last two chapters, "Advertising" and "Selling," are tag-end. After thirteen chapters dealing with various aspects of personnel, two on these important topics seem too scanty. Both chapters miss the mark. In advertising, the psychologist should provide his most valuable contribution in the selection and use of appeals (motivation). The problems of type face, trade names, color, etc., seem more matters of engineering. Moore has dealt with them in the form of psychological problems, but often a second careful reading is necessary to uncover this. Discussion of testing advertisements is good and to the point, and not too lengthy. Most of the space in

the final chapter is spent on the personality of the salesman and on other points not squarely at the heart of the problem. Selling, the reviewer believes, is primarily a face-to-face meeting between salesman and customer—even though this situation is less objective and less amenable to statistical treatment.

University of Wisconsin R. W. HUSBAND

A Basis of Opinion. By ADRIAN COATES. London, Macmillan Co., 1938. Pp. xvii, 457.

The central theme of this book is that people exist, and only things are real. Philosophy is defined as a reasonable point of view. Verbal affirmation is made in regard to the common-sense point of view. The author's own way of looking at philosophy is, of course, the meaning of common sense applied to philosophy. No claim is made to impartiality; but admission is made of a life long prejudice against mysticism, transcendentalism, and the claims of revealed religion and authority. The text throughout is a presentation of the personal, analytical view offered as a substitute for the common, objective-metaphysical outlook.

Things have no transcendental essence. Their essence is in their definition. Reality is given in experience. He supposes transcendental realities are merely poetical devices or pitfalls of hypostatization having only conceptual reality. Mind is the potentiality of action. The expression, "in the mind," refers only to remembering or being thought of. The real nature of a thing is in the nature of our perceptional experience as a whole. Reality and perception are co-relative terms. Reality is 'thingness.' "An object is real," Coates states, "because we all see it as the same object and the same kind of object." The idea that science is a search for reality must be abandoned. The universe cannot be an object of perception since the 'universe' cannot be distinguished as an 'entity' from the "universe or totality of our sensible experience." Thus things are resolved into events, material substance into eventuality. Eventuality, in place of reality, is the ultimate and all-inclusive term within the context of sensible experience.

Persons only can be said to exist. All the 'nows' or present moments and the activities of the selves are the context of actuality. The distinction between the "subject" and the "object thought of" is only logical. The subject is one part of the thinking self while the object thought of is another part. Even time, that is, real time, is a projection of actual time into the realm of logical thought and objectivity. Coates' distinction of the actual and the real seems to be a parallel of the psychological distinction of structure and function.

The concepts of causality, purpose, and freedom remind the reader of a dynamic view of behavior. In regard to causality, the main types of questions are: (1) Why is it? (2) Why or how did it happen? (3) Why did he do it? One's end or purpose supplies the reasons for his actions. Final causes belong only to the sphere of individual activity. To say that a man is free to say that he acts as he wills.

Facts are defined as being wholly within the realm of experience. To quote, "A fact is what is known to have happened. . . . Scientific fact is the experiment or controlled experience of a person working toward a definite end of scientific knowledge." Truth and knowledge are characterized in terms of certainty and conviction. As facts become more general, they tend to pass into generalizations. In belief, the degree of certainty is contextually emotional or volitional. All religious and

political belief has the emotional and volitional basis. Belief is essentially an act of will. Intellectual certainty is the basis of all knowledge.

History is more inclusive than reality. Reality is an aspect of history. We know and contemplate the past; but what we belong to is the present. The transcendental basis of history is to be found in the existence and activities of other selves. We cannot see what kind of a world we are building.

The system presented in the book is a philosophy which repudiates most of the philosophy and religion of the past. Religion and morality are regarded as being unrelated. Pragmatic value is attributed to religious concepts. Training of character is considered not as a matter of creed but of personal example.

In the main, the author is faithful to the dynamic structure-functional psychological standpoint. The manner of writing obscures somewhat the behavior situations which evidently are the background of the analysis. Throughout, the argument is consistent with the central theme. To the psychologist, the book is a sound philosophy which does little violence to the major current psychological concepts.

University of Utah M. C. BARLOW

Psicologia del Comportamento Sociale. By A. MIOTTO. Florence, Vallechi, 1939. Pp. 151.

Psychologists and sociologists have long tended to disagree in their analyses and evaluations of the relations between the individual and the social group. In extreme cases, the psychologist has denied the reality of the group; the sociologist, the reality of the individual; or, in any event, there has been a tendency to subordinate the one to the other by declaring that the individual is merely a function or product of the community or on the other hand that society is but a sum or aggregate of individuals. The evolution of this difference in point of view, says Dr. Miotto, is very interesting and the disagreement has assumed a precise form as a result of recent studies of animal sociology. Compromises between extreme doctrinal positions are impossible, and the purpose of Dr. Miotto's work is an unprejudiced examination of social behavior as a function of both individual and environment, and an attempt to define more precisely the nature of these terms.

The introductory chapter, treating instinct and affective life, aims to demonstrate the equal and reciprocal influence of individual and environmental factors at every level of biological and sociological complexity. In short, individual and environment, apart from one another, are mere abstractions. The concrete reality is social behavior.

For the purposes of analysis, instinct and reflex are regarded as belonging to the same category and are closely connected with emotion as the concluding phase of a process of excitation followed by tension and distension. Emotion in its primitive form is the emotion of static equilibrium and the biological process is in essence reaction or disturbance of the organism characterized by reflexes and instincts, followed by return to equilibrium experienced and exhibited as emotion. Such a view disposes of vitalistic and finalistic theories of animal behavior and places equal emphasis on the two poles of behavior, individual and milieu. In short, the primary activity of the organism is largely adaptation to itself—adaptation to its own structure and system of functions, in a word, the maintenance of equilibrium. But it is not solely this. At the level of psychological process a similar tendency toward equilibrium appears. The author calls it the statics of the mental life and cites

Spinoza, Freud, and Cannon in defence of the conception. The existence of mental functions extends and complicates the environment, finally creating a true social environment which becomes indispensable to the very actualizing of psychological functions. Thus individuals and milieu are equally real and necessary conditions of behavior.

Such facts lead the author to a discussion of the importance of the environment. It is clear that the continually increasing complexity of human behavior cannot depend exclusively on the individual as a biological unity since all of his activity is in essence a return to equilibrium. But from what does he return? From the environment? A vague term which needs to be very precisely defined! Indeed it is here that Dr. Miotto feels that the sociologists have erred and in failing to define and classify environment, have invited the attacks of psychologists who encounter a misuse of the term and may themselves be led astray by such misuse.

Sociology has gone on false paths through failure to analyze the notion of environment and define its types, by an obsolete conception of the process of adaptation, by the substitution of teleological speculations for observation and experiment, by abuse of the method of the biological sciences, and the reduction of sociology to a species of social physiology. The principal errors, however, are inadequate notions of the environment and adaptation and these occupy the author for the remainder of the book, but chiefly the former. With a remark that biological adaptation can no longer be defined in Lamarckian terms, Dr. Miotto turns to a classification of environments and a discussion of their inner relations and significance to social behavior. He classifies environments as cosmic, organic, social, and psychological and in chapters three and four, he offers a condensed but well documented account of their respective rôles.

Dr. Miotto next discusses the so-called social tendency of man and rejects various efforts such as that of Freud to reduce it to a single primary tendency or drive. Inter-attraction, imitation, and a sentiment of participation all contribute to impose the particular forms assumed by the forces tending to establish equilibrium between the individual and his environments.

The final chapter is devoted to the reality of the socialized individual. The general conclusion based on analyses of what seem the most individual experiences, such as dreams, mystic states and the inventive process, is that the individual is inevitably socialized. Society is woven into his essence and he could not abstract himself from it if he would. In conclusion it may be said that Dr. Miotto's work is logical, remarkably condensed, and embodies criticism equally valuable to psychologists and sociologists.

University of Cincinnati CHARLES M. DISERENS

Life: A Psychological Survey. By SIDNEY L. PRESSEY, J. ELLIOTT JANNEY, and RAYMOND G. KUHLEN. New York, Harper & Bros., 1939. Pp. 654.

The authors consider that "psychology has a primary obligation humbly to contribute whatever little it can toward human welfare" and have developed their book around the theme of this objective. They offer a comprehensive survey of sociological, economic, and biological facts as the setting for human life of today. They relate these facts to the developmental cycle which forms the common denominator of all individual life stories. Finally, they provide numerous suggestions whereby each

person may turn to his own best account the outer and inner characteristics of his life.

Part I deals with "The Conditions and Circumstances of Life." Here the cultural as well as the material environment is analyzed swiftly but carefully and recent changes are described in the social and economic condition of women, in the application of humanitarian concepts, in recreational life, in manners and in science and its point of view.

Part II traces in succession the life span developmental progressions of an individual through physical, intellectual, recreational, social, vocational, and ideological experience. This section closes with a diversified collection of case stories of people who typify the different kinds of life patterns made possible by modern living conditions.

Part III deals in detail with psychological mechanisms of adjustment. Fatigue, incentive, and emotional strain are fully discussed. This section of the book is rich in illustrative material and descriptions of concrete devices by which adjustment may be improved when it is being hampered by deficiencies of personality or environment. Special attention is given to vocational and recreational problems. The authors apparently consider these the chief problems in the field of social adjustment. One who deals with human beings as individuals rather than as bits of masses of society would have preferred to see the excellent treatment given to the vocational and recreational field supplemented by as careful a treatment of family life. There is the arena for the impact of personalities which sets the pattern with which each individual approaches the larger social world outside his home.

The most valuable phase of the book is its recurrent emphasis on the problems of elderly people and the need for greater study of their social needs and adjustment. The aged are a gift to society of the medical arts and sciences. The prolongation of life is changing the proportion of elderly people within the total population from a sprinkling of individuals to a social group. This group comes into the human race as a stranger and newcomer. Its name is on the relief rolls and it is revising the pension policies of industries. It complicates the problems of employment, increases demand for recreational facilities, and changes the complexion of family life. The middle aged have had to adjust to bearing the burden of a constantly prolonged period of childhood. If they are to be asked to carry responsibility for those who enjoy a prolonged old age, they must have help. Careful study of the needs and social assets of elderly people is imperative if a readjustment to this new group within the social order is to be made without an intervening period of chaos. The authors of this book present this field for study together with constructive suggestions for solution of some of the problems involved.

Wichita, Kansas — EDWINA A. COWAN

The Troubled Mind. By HARRY ROBERTS and MARGARET N. JACKSON. New York, E. P. Dutton & Co., 1939. Pp. 284.

This book is described as a "general account of the human mind and its disorders and their remedies." There are chapters devoted to the unconscious, complexes, emotions, habits, psychoanalysis, rationalization, sanity, neuroses, psychoses. The writers state: "This book is not intended as a textbook of psychology or of psychiatry. Rather it should be regarded as an attempt to explain, in simple language, some of the outstanding conclusions of contemporary psychologists, with such criticism as

the experience of the authors suggests. One of us has been engaged in general medical practice for several decades; the other has been for many years a medical officer at a hospital devoted to the care of mental abnormals. Such constitutes whatever authority we have."

The first 50 pages of the book do not constitute a fair sample of its worth. The historical perspective is limited. A definition of mind is attempted, the controversies of psychologists are pointed out, the structuralists are hung out on a line to dry, introspection is taken to task for its faults, and instincts are listed. The arguments are discursive and many fundamental issues are briefly, dogmatically discussed. The writers have but a speaking acquaintance with contemporary, experimental psychology.

The book improves with the turning of the pages. The chapters entitled "The Ages of Life," "What Is Sanity?," and "The Neuroses," are admirably done and suited to the needs of the laymen. An excellent chapter is the one on Freud and psychoanalysis. Although the authors state that Freud is largely responsible for the revolution in psychology and that "the debt which science must owe to him is no less than it must ever owe to Charles Darwin," yet they humorously question some of his terms, critically evaluate his more important findings, and point out the absurdities in some of the double-edged concepts. As an instance of their treatment in this connection: "To claim one never dreams at all, is not, as one might suppose, the sign of a tranquil subconscious; it merely implies that one's dreams are too scandalous for recollection. Nightmares and anxiety dreams on the other hand are symbols of a repressed sexual wish. So the psychoanalyst wins either way. One cannot help being reminded of the revivalist who finds a sin for you in the very place you didn't expect." They also point out, "that one may well feel sceptically inquisitive as to the true nature of the operative psychotherapeutic agent in a course of treatment that involves hundreds of hours of intimate personal contact with another individual."

The reader gains the clear conviction that the authors' criticisms of psychoanalysis are based on actual case experiences. Their clinical orientation combined with judicious exposition and interpretation on matters psychoanalytic make these sections worthwhile reading, to the psychologist as well as to the layman. It is to be hoped that some day the writers will find time to incorporate their exposition, interpretations, and criticisms of Freudian psychoanalysis into a separate article for more general distribution.

Rochester, New York ANTHONY J. MITRANO

La Psychologie Expérimentale en Italie. École de Milan. By A. MANOIL. Paris, Félix Alcan, 1938. Pp. viii, 489, 16 pl.

This book is an extensive summary of the work of Gemelli and his associates, research which came in large part from the laboratory of the Catholic University at Milan. Before the foundation of the Catholic University, Gemelli's studies had been concerned with neural and other bodily correlates of emotion, and with certain psychophysical problems of cutaneous space perception. From the University, whose laboratory of psychology was founded by Gemelli, came studies upon varied topics: perception (form, contour, movement, illusions of shape and size, temporal stages in perceiving); electro-acoustical analysis of language; applied psychology (selection of aviators, industrial problems, physical exercise); and comparative psychology. Altogether, Manoil has summarized a considerable body of research.

Manoil's task has been adequately carried out, and the result may be useful to those psychologists who lack facility in Italian. Descriptions of techniques and results are full and detailed, and the documentation includes references not only to the Milan group but also to many other writers in each field. A complete classified list of the references is included at the close of the book.

However adequate Manoil's summaries may be, the reviewer is struck by the question why Manoil should have undertaken such a task in the first place. The usual reason for summarizing the researches of a single psychologist or group of collaborators is either that these researches constitute integral parts of a unitary program, or that they all bear the mark of some special point of view, 'school,' or theory. A glance over the list of topics mentioned above shows that the Milan group investigated a number of widely divergent and unrelated problems. On the other hand, the common thread of point of view running through these investigations is scarcely far-reaching enough to unify the presentation.

There seem to be two main aspects of Gemelli's general point of view, and these are probably attributable, as Manoil suggests, to Gemelli's neo-scholastic philosophy. The first is an emphasis upon psychological activity as organized and directed toward a 'goal.' This is scarcely a unique position. The other point is the notion that there are 'psychic' or 'intellectual' factors determining activity, these factors being independent of physiological events. When, however, we ask what this means in terms of a concrete experimental situation, we find that it implies no more than that an activity like perception is determined by past experience, memory, 'set,' and the like, as well as by the stimulus-conditions of the moment. The assumption that such factors are 'purely psychic' is also scarcely new, and it is not an assumption which makes much difference in the interpretation of concrete experimental results at the present stage of our physiological knowledge.

This discussion is not meant to imply that Gemelli's individual researches and experimental contributions are of little value. In fact, there is a great deal of sound and careful work summarized here. But we do not need to immerse ourselves in almost five hundred pages of description in order to become familiar with Gemelli's point of view. Also, the researcher in any of the fields covered would probably find it necessary to go back to the original papers, no matter how careful Manoil's summaries are. Therefore the value of this book to psychology is scarcely commensurate with the labor which Manoil must have expended upon it.

Cornell University T. A. RYAN

Educational Psychology. By CHARLES H. JUDD. Boston, Houghton Mifflin Co., 1939. Pp. xx, 566.

To Judd, educational psychology is a study of the impact of civilizing institutions upon the maturing individual. The result of this impact is an interaction; for the individual, being adaptive by nature, is forced to conform in large measure but also being resistive insists on changing these institutions to some extent. At the heart of the educative process are language and number. The institution of language, growing as it does out of the rich experiences of the race, influences the child from the very beginning. His reactions to others, his means of influencing others are interwoven with his language development. While his personality is growing to intellectual maturity as evidenced by his ability to narrow his attention, to abstract

common elements out of phenomena widely differing in external appearances, and finally to arrive at a generalization, language helps all the way. For these reasons, the development of vocabulary, reading readiness, methods of learning to read, and procedures which stimulate reading for understanding in the upper grades receive much attention.

In like manner, the learning of number is developed. The development of the institution of number is traced from primitive counting through Roman and Arabic numerals to the higher reaches of the mathematics. This is followed by a study of the psychology of the individual's learning to count, to add, and to master the number system. Along with the procedures used in the acquisition of these number concepts appear the processes of abstraction and generalization which are nicely illustrated in those aspects of mathematics commonly taught in the upper levels of the elementary school and throughout the high school. Handwriting, the social sciences, the natural sciences, and the fine arts are treated in the same general manner with minor changes when necessary.

The greatest difficulties in learning arise out of the immature individual's attempts to learn the more generalized concepts of these institutions without having had the concrete experience necessary to understand them. Money, for example, is a generalized concept. It implies that value has been reduced to a common denominator. Only slowly did the race after various experiments with metals of various kinds, with the barter system, etc., arrive at the concept of money. The pedagogical danger is that the growing child on meeting this apparently simple but really very complex idea will learn to manipulate it properly but will not understand its nature.

The last two divisions of the book on personality and the solution of educational problems seem commonplace and uninspired after the climax reached in the treatment of language and number and through them the psychological processes of abstraction and generalization.

The book appears tc the reviewer as partial and one-sided. So close has the author been to one school of thought largely inspired by himself that other work equally valuable is treated in a cavalier manner. For example, the word "motivation" does not appear in the index. The broad field of interest is dismissed in a few pages and the problems of growth and maturation are inadequately considered. It is clear that to this author education is for the most part an intellectual process by means of which the maturing individual learns to understand the civilization which encompasses him.

This text is undoubtedly a significant contribution to the field of educational psychology. Its style is learned and intellectual; its arrangement logical and compelling; and its illustrations full of point.

University of North Carolina A. M. JORDAN

Lehrbuch der Psychologie. By TH. ELSENHANS. Third completely revised edition by F. Giese with the collaboration of H. W. Gruhle and F. Dorsch. Tubingen, J. C. B. Mohr (Paul Siebeck), 1937-1939. Pp. xii, 588.

This latest edition of the Elsenhans systematic treatise, issued serially in five paper covered brochures over a period of several years, was indeed a labor of love. First undertaken by Giese until he was stricken with an ocular disability, then finished by the two other collaborators named above, the work provides in one volume an unusual synthesis of psychology as a scientific whole from a somewhat

unbiased and non-partisan point of view and in the tradition of Wundt. The backdrop for the entire presentation is the field of natural science. The revision brings together in an uncommonly successful manner widely divergent points of view. The introspective methodology is accepted with due consideration for its difficulties side by side with the behavioristic techniques and the moot question of mind-body relationship is laid aside in terms of the biology of the personality, or, in other words, with regard for the *life* of the individual.

The chapter headings convey the extensive nature of the treatise: psychology as science, perception, thought, feelings, will, bio-functions, development of the mental life, personal integration and mental community. The book is illustrated for the most part with new diagrams and figures. It contains extensive bibliographies at the close of each section and the usual subject-index at the end of the entire volume.

In a treatment of psychological data and theory which appears comprehensive in point of view, we regret that with very few exceptions the references are limited to German sources. It is as if contributions to psychological science came from only one country. Certainly in equally broad treatments of the subject in America citations commonly transgress the national boundaries because science aside from unfortunate linguistic handicaps does not recognize geographic or political limitations. Some terminological transformations are doubtless necessary as in the case of 'decibel' becoming 'phon,' but certainly Troland's contributions to perceptual functions might call for a cross-reference somewhere, as might the work of Thorndike, Carr, Lashley and McGeoch on learning. Such expectations are all the more warranted when as many as four psychologists have had a part in the completion of the treatise.

In the recognized Leipzig tradition the book exceeds the bounds of many similar systematic works of more recent date in consideration of subjects that are not strictly scientific. The book ends with a section on the psychology of the German people which analyzes the peculiar mental traits and psychic products of these nationals as contributions to the present sociological level of civilization. Even the characteristic topographical features of the general landscape, contrasted, for example, with that of the Scandinavian peoples, are discussed as a potent influence on the socio-economic and the mental life of the Germanic population. Similarly geographic conditions, such as being surrounded by neighboring lands as distinguished from an insular position, provide much thought and discussion in this direction.

Scholarship, clarity of diction and attractive presentation of factual materials run hand in hand throughout the book. A 'still' hunt for the obscure and the obtruse would fail to bag much intellectual game of that species. At the same time theoretical attitudes on controversial subjects seem for the most part well balanced with no apparent desire to proselyte the reader unduly. It is truly a valuable reference work which brings the German systematic facts and fields of psychology into perspective from the present day angle and which gives us in addition an accumulation of interesting experimental facts not usually so accessible to the American scholar in psychology.

Chicago, Illinois CHRISTIAN A. RUCKMICK

Facts and Theories of Psychoanalysis. By IVES HENDRICK. 2nd ed. New York, Alfred A. Knopf, 1939. Pp. xiv, 369.

Dr. Hendrick's book is in major part a précis of Freud's teachings, and in itself is hardly subject to criticism save as a fair and reasonably full account of these.

To the best of the reviewer's belief it is so. In style clear and temperate, it can be well recommended to psychological readers sharing a skepticism long current, at least outside clinical areas. This applies most to Parts I, III, and IV, rather less to Part II, which is much absorbed in technicalities and abstractions, with chief interest to the special student. Disagreements there will be at best; a possibly underestimated source may be noted here.

Many years ago the reviewer observed in effect to the author of this book, that one got a better impression of psychoanalysis from the analysts' conversation than one did from their literature; and despite such accounts in English as Hendrick's and Martin Peck's, this has not ceased to be the case. It seems to originate in a very easy but not too obvious confusion of Freudian psychology and its rhetoric, for which the former comes out much the worse. It was and is bad rhetoric to speak of psychoanalysis as a science, or to use the terms sex and wish in the highly abstracted meanings given them. This does not make the observations bad psychology, but critics are very pardonable who ask themselves how far those who are so indifferent with the instruments of understanding, are likely to have good understanding of what they observe. It is for the analysts themselves to resolve these matters, and the worth of their message will not be duly recognized until this is done.

Among the least happy of these usages is that involving the transitive sense of "psychoanalyze." The implication is, that the analyst performs some esoteric process, by which the person analyzed is benefited. The teaching, clearly embodied in Hendrick's book, is that the patient or student analyzes himself, with the help of the analyst, who in the Freudian sense, performs rather the functions of a catalyst. This explains Hendrick's observation, that those in a position to get the most from the analytic experience are often the honest skeptics. The conversational remark of a similarly brilliant colleague of Hendrick's, "distrust anyone who is *enthusiastic* about psychoanalysis," was meant for protagonists but applies not less to the clientele. It is enthusiasm for the analyst's doing the real work. *Sensu stricto* one cannot 'be psychoanalyzed' any more than one can 'be' confessed. We need here the middle voice of the Greeks; one makes his confession, and makes his psychoanalysis; in either case with someone capable of facilitating the process, but having a far from sufficient rôle in accomplishing it. A deeper understanding of this would greatly help the attitudes toward psychoanalysis "of them which are without."

Any book of this kind needs a glossary, but the present one is not among its best-considered achievements, at least for non-analytic terms (*e.g.* pathological = abnormal). In this sophisticated age it verges on impertinence to explain for the putative reader such terms as therapy, gastric, gratification, heredity, not to say anus, coitus, defloration, menopause. On the other hand, knowledge of the vernacular would suggest finding some more univalent term for lay analysis, even though the local profession be "against it."

Harvard University F. L. WELLS

Biology and Christian Belief. W. OSBORNE GREENWOOD. With a foreword by Theodore Savory, Malvern College, New York, Macmillan Co., 1939. Pp. 192.

It is doubtful whether this book would merit the critical attention of a scientific journal were it not for the fact that it contains a pattern of ideas which is characteristic of an increasing number of cults, movements, pronouncements, and books. In these the findings of modern natural and physical sciences are made to appear

meaningless without the postulation of a Master Mind. Although the level of intellectual sophistication varies from the crudest forms of evangelical religion to the comparatively erudite formulations of such philosopher-scientists as Eddington and Jeans, the theme is the same: In the study of Nature and Man an *impasse* has been reached; the only hope in the blooming, buzzing confusion is a return to a faith in transcendental, mystical principles. And, so the argument frequently runs, since the scientist has been forced to resort to the supernatural, so must the man-on-the-street seek supernatural direction in the solution of his economic and political problems. At which point, of course, a whole political and social doctrine is implied.

Whether these manifestations are the final convulsive spasms which precede the death of formal religion as we have known it for the last 100 years, or whether they are among the premonitory signs which mark the attempt to abandon, or at least discredit, rationality as a basis for human action, it is perhaps too early to say.

The present volume covers everything from the nature of electrons (which turn out to be essentially spiritual in principle), to the nature of human personality (which turns out to be the one characteristic of man that "has all the qualifications for establishing and maintaining contact with a personal God"). There are chapters on Purpose, Glands, the Nervous System at Work, Survival of Death, and, of course, Psychical Research. The author apologizes in the Preface for the omission of a discussion of Anthropology and Miracles.

Sir Oliver Lodge, Jeans, General Smuts, Wm. McDougall, Eddington, Chas. S. Myers, and Charles Richet are among those who furnish comforting validations, while Sherrington, Darwin, Bertrand Russell, and Morton Prince seem to have made statements which, at least, do not contradict the major thesis of the book.

This thesis may be summarized in two propositions. (1) The evidence of modern science clearly points to the existence of a "directive Mind," an "architect who has thought out the whole scheme before ever a line, so to speak, was put on paper." (2) Physical, biological, and social phenomena may only be understood as inseparably linked with Guidance and Control exerted by a personal God. The use of the terms "guidance" and "control" bears a striking resemblance to that of certain contemporary religious movements—notably the so-called Oxford Group.

From the point of view of contemporary physiological psychology, the chapters on the nervous system are almost incredibly naïve. A strawman is created in the form of a "mechanistic biologist" who has the strange belief that the brain "secretes mind." On the contrary, says our author, the "seat" of the mind is in the frontal lobes which contain merely the machinery through which mind manifests itself. The "mental faculties" are not localized in brain tissue, for the reason that "mind cannot be caged within the limits of a mechanical contrivance," but it is able to manifest itself only in this particular part of the brain. All this can be understood by accepting "the great Creative God Mind—the one element in the whole cosmos which confers reasonableness on a world which otherwise too often seems irrational."

It is the judgment of the present reviewer that this book is not only an example of distorted science and bad logic, but insofar as it is representative of those attempts to discredit man's competence to understand his universe except by the introduction of the supernatural, it conveys a threat to any social order which is erected on a rational basis—a threat, which at the present stage of the world's history, it may not be safe for the responsible scientist to ignore.

University of California (Los Angeles) FRANKLIN FEARING

Our Sex Life. By FRITZ KAHN. Translated from the German by George Rosen. New York, Alfred A. Knopf, 1939. Pp. xxxviii, 459, 41 figures.

The author of *Our Sex Life* is a physician, a practicing specialist for more than twenty-five years. The book is intended to supply practical information on sex. An extensive range of topics is presented including chapters on hygiene, fertility, contraception, and impotence. The style is felicitous and easily comprehensible. The many illustrations are well done. Although Kahn does not hesitate to stress pathological consequences, morbidity is avoided. Occasionally the author indulges too freely in poetical and romantic phraseology, but, for the most part, the attitude is objective and matter-of-fact. Sex is treated as a specifically human function without reference to its rôle in evolution or its place in biology.

The emphasis is psychological and pedagogical as well as medical. The influence of psychoanalytic contributions is evident but the categorical explanations of psychoanalysis are not adopted. The significance of psychological factors is stressed throughout the book, particularly in the chapter on impotence.

The most noticeable shortcoming is the extreme tendency to exaggeration. For example, Kahn states that "thirty per cent of all mentally inferior children who are provided for in institutions are syphilitic"—a statement in agreement, perhaps, with popular thought but contrary to statistical fact. The exaggerations are abundant in other connections and the reader is constantly compelled to question critically the evidence.

Nevertheless, the book serves its purpose well. It is distinctly better than the usual book on the subject. The cost of the book is, however, too great to permit distribution to the general reader for whom it was intended. This is unfortunate, in a way, since the book's usefulness will have been outlived within a few years when other writers will want to incorporate some of the important psychological findings of recent investigations such as may be found in Terman's *Psychological Factors in Marital Happiness.*

Rochester, New York ANTHONY J. MITRANO

Man Against Himself. By KARL A. MENNINGER. New York, Harcourt, Brace & Co., 1938. Pp. xii, 485.

Menninger presents his theme, that "each man kills himself in his own selected way" (vii), in six parts, under the titles: destruction; suicide; chronic suicide; focal suicide; organic suicide; and reconstruction.

The first part deals with a great paradox. Man has his hands full fighting Nature, but not satisfied with this struggle alone, he fights his fellow men, and not infrequently turns against himself. Thus life is a continuous battle between the life instinct and the death instinct.

In the United States alone, about 22,000 people take their own lives every year, while in some European countries suicide is more than twice as frequent. Yet, very little is done about the problem outside of the medical profession. Menninger shows how difficult it is for physicians to overcome public lethargy and save the lives of would-be suicides.

The question that naturally arises is why there should be so many suicides when the urge for self-preservation is supposed to be one of the fundamental laws of life. According to popular opinion, "suicide is an escape from an intolerable life situation" (p. 17). According to Menninger, however, much of the motivation of suicide

is not external, but is found in the unconscious. It consists of the wish to kill, the wish to be killed, and the wish to die.

Besides the suicide that causes death all at once, there are others that bring about death by degrees. Menninger calls such suicides chronic. It is a "suicide by inches as it were" (p. 87). Asceticism, martyrdom, neurotic invalidism, alcoholic addiction, anti-social behavior, and psychosis are examples of chronic suicides.

The third type of suicide is focal suicide. This type limits itself to specific parts of the body, and by bringing about the destruction of these often causes the destruction of the entire organism. The phenomena that belong here are self-mutilation, malingering, compulsive polysurgery, certain unconscious purposive accidents, and sexual impotence.

Finally, the fourth type of self-destruction is organic suicide. As repeatedly indicated by the psychoanalysts, man is in constant need of love. If this love is not forthcoming, the individual concerned may come to hate the person from whom he expected love. In such instances the individual who seeks to express his hate, but cannot, may turn this hate upon himself, and as a result invite organic suicide. The hate thus achieves expression in the guise of bodily diseases which may eventually cause death.

Menninger suggests that man should try to sublimate tendencies which are not compatible with the conditions by which he is bound. Man is free to avail himself of the clinical and social techniques in this matter, and need only half try to fight the death instinct in himself to be successful. "Aggression," says Menninger, "should be deflected to harmless targets" (p. 442).

Menninger has dipped freely into his rich experience as a psychiatrist to illustrate the many points in his exposition, and despite occasional overstatement, the book presents a well reasoned case. It focalizes man's tendency towards self-destruction and shows the way to combat it.

University of Manitoba PETER HAMPTON

Public Opinion. By WILLIAM ALBIG. New York, McGraw-Hill Book Co., 1939. Pp. xiii, 486.

The era of the War of Nerves assuredly calls for a calm, dispassionate, and scholarly review of the broad field of public opinion in the light of controlled investigation. Even a reviewer who was chary with his superlatives would be forced to grant that Albig has accomplished this in a volume which is not likely to pass from sight within the next few years.

A study of communication supplies the common thread of the exposition. After defining 'public opinion' and 'communication' the author proceeds with a discussion of the various influences which serve to mold public reaction. This carries him through such varied fields as the study of myths, the analysis of leadership, the measurement of opinions and attitudes, and the investigation of changes of opinion. He deals with propaganda and with the interest groups which employ special pleading. He introduces the reader to basic data concerning the status and the known effects of the radio, the moving picture, and the verbal and graphic contributions of the press. He does all this with a minimum of special pleading on his own part, introducing instead empirical data and thoughtful analyses. Even when he deals with the topic of the effect of violence upon public opinion, he resists the temptation to become violent himself and continues to write as a democratic liberal.

It is possible to cover within 400 pages as wide a field as he does only because he writes in a highly compressed style which verges occasionally on the staccato but which is always fluent and readable. Albig has read widely and critically; his ability to digest what he has read and to relate it to the news of the day is most impressive. As must be the case, there are spots in which the exposition falls short of the level of excellence maintained through most of the volume. The sections on propaganda, for example, lack the breadth and thoroughness of many of the other chapters. Such shortcomings are easily forgiven, however, when one realizes that the author never plays the pedant, never moralizes, and never uses his illustrations as anything more than illustrations. Nor do they detract seriously from the importance of the contribution that Albig, himself a sociologist, has made to social psychology.

University of Maryland JOHN G. JENKINS

Der Arzt in der Dichtung der Gegenwart. By BRUNO WACHSMUTH. Enke, Stuttgart, 1939. Pp. 241.

A large part, but not all of the book, consists of epitomes of novels in which a physician appears as the principal personality and of novels written by physicians. It is therefore in a way a contribution to the history of medicine. The author, however, is a historian of general literature by profession. Around 1890 medicine was a kind of lighthouse for all intellectual activity at least in Germany, if not in the world. The romanticism of the beginning of the century was now looked upon as a kind of medieval darkness. For example, Strindberg wanted to be a physician before he found his vocation in literature, and Ibsen prefers a physician to any other professional man as a spokesman for his ideas. At the end of the nineteenth century the physician was the ideal man. The author points to these facts by numerous quotations from and analyses of literary works. It is impossible in a review to give a correct idea of the wealth of the material thus presented.

With the beginning of the twentieth century realistic criticism applied to the practice of medicine comes to the front. The specialist is pictured in literature as a suspicious, questionable character. "Naturopathy" is praised. On the other hand the physician is also often pictured as the *Führer* of the population. Conflict of the physician with governmental authority occurred occasionally. The author mentions the case of the President of the 1904 meeting of the German Society of Physicians and Naturalists, who was severely censured by the Prussian government for having permitted the reading of a paper which denied a personal god and an immortal soul and asserted that Science would lead to universal tolerance and international peace. Later the censured president said in his autobiography that the troublesome paper had unnecessarily spoken of what was everybody's common knowledge. The world war produced a great change, which the author expresses in the words that "it was Science which lost the war." In 1929 one could hear official medicine give utterance to the notion that "he who does not believe in the psyche can not be a physician."

The final chapters of the book contain numerous abstracts from literature in which the often tragic conflicts of the physician's professional ideals with his sentiments as the father of his own family are described, especially in central Europe during the decade following the world war.

University of Miami MAX F. MEYER

Mensch und Volk der Grossstadt. By WILLY HELLPACH. Enke, Stuttgart, 1939. Pp. 139.

The social problems created as well as solved by the increasing tendency of populations to live in large cities are here very ably discussed. Older evaluations of the civilizing character of a metropolis have often been negative. But the objective studies of more recent social science have caused such negative estimations to become more and more rare. The author, a prominent psychiatrist and anthropologist, is unusually well qualified to present both the advantages and the disadvantages for the individual as well as for society of the crowding together of hundreds of thousands of human beings in one spot. Right in the beginning he quotes a famous parliamentary speech of the feudal landlord Bismarck, who could not understand why one of his "very decent" hired men, after military service in Berlin, refused to return just because, as he said, in Berlin he could on a Sunday drink a glass while sitting in the air of a beergarden and listening to music with a thousand other citizens—as if the country did not offer him "the open air" every hour every day all the year around. But the city gives, combined with an unenforced sociality, a freedom which is denied to him who lives in the country. Hellpach discusses the city man from every point of view in physical anthropology and finds that he is far from being mentally or physically inferior to his country brother.

The second subdivision of the book shows in detail how the city in a psychophysical sense makes a different being out of him who left the country. Climate, different nature of sunlight, the very ground on which the city stands, food, clothing, bedding, a variety of poisons, rapidity of maturing are factors which make the city population different from that of the country.

The third part gives the social psychology and characterology of the city man. In many different ways the city makes the man free. Marriage is delayed. The number of children per family is reduced. If these and other facts are regrettable, the author insists that they can not be modified by moral indignation and condemnation (just as syphilis could not be abolished by such means) but only by a frank scientific study of the causes and measures for amelioration based upon such studies. Any national language is largely the result of the influence of one or a few large cities. Without large cities that unifying element would be absent and the dialects alone existing could hardly be mutually comprehensible.

That there is much in a large city which is unnatural, abnormal, the author not only admits but emphasizes; and for that very reason he emphasizes the need of a thorough scientific study not only of a few, but of all the phases of city life.

University of Miami MAX F. MEYER

Aesthetic Motive. By ELISABETH SCHNEIDER. New York, Macmillan Co., 1939. Pp. 136.

Miss Schneider takes "the unity-in-variety theory, being as old as any in the world" (p. 129), and finds that such unity gives man pleasure because "one of his most disturbing problems . . . is his desire for unity when faced with the multiplicity and otherness of the world" (p. 38). Thus she hopes to find the key to esthetic experience in an understanding of human motivation and need. That the esthetic problem, which has so long resisted solution, might be profitably attacked by way of a study of the esthetic motive is a view that will appeal to the modern

psychologist, but Miss Schneider produces little evidence to support her particular thesis. She rejects both the experimental method of the psychologist and the empirical method of the philosopher, and holds to what I take to be the hit-or-miss method of the artist—that is to say, she paints the truth as she sees it about esthetics, frames it in covers, hangs it in your library, and asks whether it does not please you by introducing unity into the variety of your thoughts about esthetics.

It is natural that the esthetician should use the artist's method of appeal to conviction, and also that the scientist should resent that procedure, asking for a weighing of evidence. Miss Schneider does not say how that conflict of attitudes toward knowledge could ever be resolved, and I, believing in evidence and mistrusting conviction, find her many dogmatisms disturbing. For instance: "The esthetic impulse appears to be exclusively the possession of man" (p. 12; cf. also pp. 38, 129 f.). If my conscience could free me for equally dogmatic expression of conviction, I could oppose to Miss Schneider's anthropocentric view: "Man has never been shown to possess a general psychic capacity that is not shared by lower animal forms, and it is doubtful if most human mental capacities are well understood until their animal forms have been described." I could even marshal evidence for that view—evidence from the volition, thought, reasoning and language of animals. In fact, the world needs an animal esthetics, but it will not be Miss Schneider who gives it to us.

Harvard University EDWIN G. BORING

Allied Propaganda and the Collapse of the German Empire in 1918. By G. G. BRUNTZ. Stanford University, Stanford University Press, 1938. Pp. xiii, 246.

"When, therefore, all allowances have been made and all extravagant estimates pared to the bone, 'the fact remains, propaganda is one of the most powerful instrumentalities in the modern world.'" Thus Bruntz quotes Lasswell in concluding his survey of the effects of allied propaganda upon the Germany of 1914-1918. In a meticulously documented and scrupulously unprejudiced review of many hundreds of specific examples of propaganda, the author leaves small doubt as to the truthfulness of his quotation.

After a review of organizations and general tactics involved in the barrage of war-time propaganda, Bruntz proceeds with his analysis of the types of materials employed. He discusses propaganda of enlightenment, of despair, of hope, and of a particularist (*i.e.* sectionally-aimed) sort. Revolutionary propaganda is sufficiently important to warrant a separate chapter. As a primary contributory condition the internal situation in Germany is subjected to careful scrutiny. A chapter on measuring the effect of propaganda proposes and discusses a half-dozen 'yardsticks' by means of which the influence of materials and methods may be assayed. The volume closes with an extensive bibliography.

The volume is characterized by the presentation on almost every page of original documents and citations from primary sources. Although the translations of some of these remind one of the efforts of a non-linguistic sophomore, and although the punctuation of them is distinctly impressionistic, such minor faults cannot detract from the generally careful workmanship and praiseworthy coherence of the volume as a whole. Bruntz's work is definitely on the *must* list for social psychologists and there are few psychologists of any sort who will fail to obtain a clearer insight into the present European situation by reading it.

University of Maryland JOHN G. JENKINS

Educational Psychology. By JAMES L. MURSELL. New York, W. W. Norton & Co., 1939. Pp. x, 324.

Mursell's thesis is "that mental life must be understood as a dynamic process of interaction between the individual and his environment." He strongly emphasizes the concept of mental growth.

In the early part of the book, he places educational psychology in its relation to general psychology and to education. Following this the usual topics are considered: heredity vs. environment; individual differences and adjustment; mental growth and development; the nature of learning; types of learning; transfer of training; the social setting of mental development; and the dynamics of learning and growth. In his treatment of the last topic the author summarizes his views and attempts to develop a philosophy of life in his readers.

The book is easy to read. It is interestingly written and is richly supplied with illustrative examples. A selected list of references for further reading is appended to every chapter. Few technical terms are used and then in a way that makes them understandable to the beginning student of education.

The author does not stress any of the so-called theories of learning enough to bias a reader. He does not take sides in many matters and has left out much material that is considered in most texts, because he does not think it is of enough importance to undergraduate students or beginning teachers, for whom the book is written.

Baltimore, Md. DONALD R. SMITH

The Schizophreniform States: A Katamnestic Study Based on Individual Re-examination. By GRABIEL LANGFELDT. London, Humphrey Milford, Oxford University Press, 1939. Pp. 134.

Through a personal reëxamination of 100 discharged patients who were admitted to the Oslo Psychiatric Clinic as doubtful schizophrenics, Dr. Langfeldt attempted to show that a finer diagnostic sifting of this material is necessary for its successful treatment. He sought, in the present monograph, to establish (1) the pre-psychotic factors which characterize this group, (2) the ways in which the psychoses developed, (3) the duration of the psychoses before admission to the institution, (4) the relation between the duration before admission and the future course of the disorder, (5) the atyptical conditions, and (6) their influence on future development. These facts are presented in numerous tables and are illustrated by aptly chosen case histories.

Dr. Langfeldt's study discloses that the typical schizophrenias and the schizophreniform states (atypical) are fundamentally different in their prognosis. About two-thirds of the latter are, he finds, cured spontaneously within the first year after discharge from the clinic. They, moreover, are permanently cured.

Dr. Langfeldt believes that shock therapy must be judged according to whether the cases are typical or atypical. He holds that the bulk of his cured cases present a symptomatology of a schizophreniform character similar in phenomenology to the type of case which reacts favorably to cardiazol and insulin treatment.

For the students trained in America, such statements as "he appeared to be somewhat imbecile . . . IQ 75" (p. 43) will seem strange. A stilted translation provokes, moreover, much re-reading. Investigators using the method of shock-treatment will, however, find much of interest in this monograph.

Lewisburg, Pa. ROBERT M. LINDNER

Principles of Psychology. By FRANCIS L. HARMON. Milwaukee, Bruce Publishing Co., 1938. Pp. xiv, 609.

Professor Harmon's book is intended as a text in general psychology for students in Catholic colleges, and is one of the Science and Culture Series edited by Joseph Husslein, S.J.

The author's religious affiliations and convictions presumably account for his definition of psychology as "that division of philosophy which deals with the human soul" (p. 3), and for his unremitting efforts to persuade his readers that human beings have a soul, that the soul reacts upon the body, that the soul is immortal, that animals lack "rational powers," and that the will is free. Adversaries of these Scholastic doctrines are vigorously and repeatedly attacked throughout the book, though Dr. Husslein may err when he states in the preface that a "refutation" of their views has been achieved (p. ix). The materialistic hypothesis is asserted to be responsible for the "low esteem in which contemporary psychology is held by many leaders in other fields of science and philosophy" (p. 72), and for the fact that the "man in the street" is often much more successful than the psychologist is in understanding and controlling others (p. 89).

As Professor Harmon frequently admits, most psychologists do not accept his philosophical premises. Nevertheless, from the viewpoint of the somewhat limited audience which he is addressing, the text deserves commendation. There are numerous references to contemporary experimental literature. There is an abundance of original and therefore unstereotyped diagrams and drawings. In particular, the clarity and vigor of the style testify to the possession of dialectal and literary skills to a degree none too common among psychologists. Perhaps, however, these stylistic merits derive in part from the simplicity of the author's dualistic system, and from the ardor with which he expounds and defends it.

New York University LELAND W. CRAFTS

Dreams and Dreaming. By H. TASMAN LOVELL. Sydney, Dymock's Book Arcade, 1938. Pp. viii, 128.

This small volume is a new edition of Monograph No. 2 of the Australasian Association of Psychology and Philosophy. The first part is an exposition of Freud's theory of dreams and dream mechanisms. Most of the illustrative material is in the form of quotations from psychoanalytical literature, particularly the better known writings of Freud and his disciples. Although the author might be classified as a Freudian, he does criticize certain conceptions of the "master." He attempts to reconcile traditional psychology and psychoanalysis, principally in terms of the psychology of McDougall. He agrees with Jung's and Maeder's stress upon the significance of the manifest content of the dream. He agrees with Rivers in believing that other emotional states than wishes are basic in dreams and River's criticisms of the importance of sex in the Freudian interpretation.

Unfortunately the author's attempted reconciliation of traditional psychology and Freudian psychoanalysis does not consider those few experimental studies which have been conducted. For instance, in discussing the influence of sensory stimulation upon dream content, he cites anecdotes and does not even mention the experimental work done by Horton and Cubberly.

This volume will serve as an introduction to a very slightly modified Freudian

theory of dreams. For the professional psychologist there will be little of value in the work except the author's attempt to reinterpret dream mechanisms in less personalistic terms than Freud does. The appendix on dream prophecy probably will not add to the admiration which psychologists will hold for the volume.

University of Newark FREDERICK J. GAUDET

Collection and Presentation of Statistical Data in Psychology and Education. By MARTIN F. FRITZ. New York, Prentice-Hall, 1939. Pp. vi, 58.

This little manual is intended to supplement textbooks on statistical method and on tests and measurements by describing in a very simple way some of the procedures to be followed in the collection, tabulation, and graphic treatment of data. The discussions of the common sense of method and the interpretation of graphs are well done. The usefulness of the material to the beginning student will depend largely upon his lack of familiarity with statistics and, perhaps, upon his inability to comprehend the mathematical concepts presented in the usual textbooks.

The manual has been carefully prepared and the author renders service by frequently pointing out sources of error in interpretation, as, for instance, in the use of area and solid diagrams and in the uncritical use of percentages. While this material is scarcely new to graduate students in psychology and education, the manual does provide additional illustrations which will doubtlessly facilitate understanding.

As a final word of advice, the author suggests that students who are unable to solve their problems write to a statistician in some university. He adds, pertinently, that these people are usually busy. It is to be hoped that students will remember this admonition when following the author's advice.

Columbia University HENRY E. GARRETT

Superstition and Society. By ROGER MONEY-KYRLE. London, Hogarth Press, 1939. Pp. ix, 163.

The purpose of this book is to propose and initiate a liaison between psychoanalysis and anthropology in the study of society and social issues. Comparative studies of racially similar tribes show that children who are sexually suppressed and disciplined are more fearful and anxious than those from tribes in which the libido is unbridled. As adults they are less coöperative and democratic, but more cruel, intolerant, and ambitious than petted children.

The origin of the childish anxiety motive is explained in typical Freudian fashion. The babe's fear of his father compels suppression of his incestuous urge until this inhibition is fixated in the personality as a Super Ego. Much anthropological significance is attached to the indirect resolution of this childish conflict in myth and ritual, and in the neuroses. Also in religion and the modern obsessional drive to work and succeed. The irrationality of modern economic and international conflict is interpreted as a struggle against various symbols of the tyrant father.

The interpretation is as valid as the orthodox Freudianism which it espouses. In the writer's opinion the great gift of psychoanalysis to psychology is its personalistic dynamics. This gift is enhanced by the fact that the agents of action wear masks.

University of Colorado THOMAS H. HOWELLS

Representative Psychologists. By ANTHONY C. WESTERHOF. Union Bridge, Maryland, Pilot Publishing Co., 1938. Pp. vi, 119.

The psychologists selected for consideration are Stern, Koffka, Köhler, Lewin, Terman, Tolman, Freud, Jung, Adler, Pauly, and McDougall. Pauly, whose name may be unfamiliar, is a German Lamarckian biologist. These men are said to represent "the present leading schools:" the Personalistic, the Gestalt, the Psychoanalytic, and the Hormic. The Purposive Behaviorism of Tolman and Terman's position are not definitely classified.

Dr. Westerhof does not seriously undertake to report experimental or clinical observation. He is interested almost exclusively in philosophical views, stated or implied. More specifically, he is interested in establishing the positions of the representative psychologists in relation to an assumed controversy between mechanists and teleologists. His own views are avowedly teleological and this causes him to favor the interactionist conception of the mind-body relation and the belief that consciousness is a determiner of behavior. He approves Terman's insistence on the reality of purposive striving, Pauly's pan-psychism, and most of McDougall's system of theory. The main issue is not met by the various Gestalt writers, he feels.

Dr. Westerhof's literary style is extremely attractive and his characterization of the various philosophical views is penetrating and, at times, humorous.

University of North Carolina ENGLISH BAGBY

A Manual of Psychology. By G. F. STOUT. Revised, in collaboration with the author, by C. A. MACE, with an appendix by R. H. Thouless. London, University Tutorial Press, Ltd., 1938. Pp. xix, 708.

This is the fifth edition of Stout's well-known *Manual.* It is a reprint of the fourth edition which was revised by C. A. Mace. The changes from earlier editions, according to Mace, are chiefly four: (1) a restatement of Stout's general theory of the relation of thought to sentience; (2) certain amplifications dealing with the relation of psychology to philosophy; (3) indications of agreement between Stout's theory and Gestalt psychology; and (4) the relation of Stout's doctrine of conation to that of McDougall.

The fifth edition differs only in the addition of an appendix by Thouless giving a brief, though adequate, statement of Gestalt theory, followed by a supplementary note written by the author in which he states his dissent from Gestalt psychologists on two points: "(1) their almost complete denial of the part played by past experience in perceptual process; (2) their neglect of the difference between what I call sensible appearance and perceptual appearance."

Cornell University R. M. OGDEN

Hegel über Offenbarung, Kirche und Philosophie. By GUSTAV E. MUELLER. Munich, Ernst Reinhardt, 1939. Pp. 61.

Mueller, professor of philosophy in the University of Oklahoma, attempts here to study the religious contents of Hegel's thinking. Students interested in the psychology of religion will find here an objective presentation of Hegel's personal problems. How objective idealism and religious belief can be expressed in one personality, Mueller clearly explains.

New York City ERNST HARMS